Contemporary Psychology

A JOURNAL OF REVIEWS

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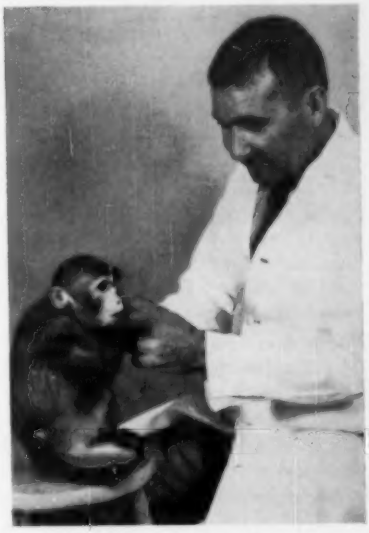
A Big Brawny Brainy Book from Texas

Daniel E. Sheer (Ed.)

Electrical Stimulation of the Brain. Austin, Texas: The University of Texas, 1961. Pp. xiv + 640. $17.50.

Reviewed by ROBERT GALAMBOS



DANIEL E. SHEER & FRIEND

Daniel E. Sheer, editor of the present volume, received his graduate education in psychology at the University of Michigan, but while there he also consorted regularly with the people who worked in the Laboratory of Comparative Neurology. From 1948 to 1950 he was a U. S. Public Health Fellow and worked on the Columbia-Greystone Project and on the New York State Brain Project. Since 1950 he has been Professor of Psychology and Director of the Psychology Laboratory at the University of Houston. The reviewer, Robert Galambos recently joined the faculty at Yale University as the Eugene Higgins Professor of Psychology and Physiology. In the 30's he received both his bachelor's and master's degree in psychology from Oberlin College, where he worked with R. H. Stetson. He went on from Oberlin to Harvard for a PhD in physiology, during the attaining of which he found time for much contact, both highly intellectual and highly social, with the local psychologists. From Harvard he moved to the University of Rochester for a medical degree. Incontrovertible evidence that he is good at many things other than the attaining of degrees is furnished by his productive ten years as Chief of Neurophysiology at the Walter Reed Army Research Institute. During his career, a large proportion of which still lies ahead of him, he has been at various times an instructor in physiology, an assistant professor of anatomy, a research fellow in psychology, a tutor in biochemistry and an interne in medicine.

FOR a long time I could not understand why the editor of *CP* asked me to review this book. Having only just recently applied for membership in APA I am therefore not qualified to speak as a psychologist to psychologists. Finally the answer hit me. Of the 42 different papers in this book only 18 list bona fide psychologists as senior authors. Yet the remaining 28 papers all treat exactly the same problems in practically the same way. Hence you don't have to be a psychologist to read this book (or review it), but if you happen to be one you can read it without violating any union rules. And many serious psychologists will read this book, I'm sure, just to see what those 18 colleagues of theirs have on their minds.

This book is edited by Daniel Sheer, a bona fide, who is also the author of three of its chapters. It must have been his idea to hold the interdisciplinary conference at Houston, Texas in 1956 to generate the book's material, and putting together what was said there, along with additional materials, must have occupied most of his spare time during the past five years. Let me try to show what he has created by examining his own contributions; through Sheer's words, so to speak, will we come to know this book.

First of all he writes the Introduction, a bit of behavior obviously appropriate for an editor. He tells us what is to come by means of a historical-philosophical essay that begins one billion years ago and ends with what Beach et al. said so elegantly about Lashley in 1960. With a picture that large to paint, and only 18 pages to paint upon—of which, incidentally, four are given over

to portraits of 16 leading figures in the field of nervous system research—the end result is bound to have some deficiencies here and there. Certainly this Introduction will not compete for completeness with the *History of Experimental Psychology* through which the esteemed former editor of *CP* added to his fame. Furthermore, much of what it covers was recently dealt with by Mollie Brazier in Vol. 1 of the *Handbook of Physiology*. What is wrong with the Introduction is what is wrong with the whole book: it is not a complete story, and much of the story can also be found elsewhere. Nevertheless, Sheer has, I think, created something interesting and useful by focusing himself, his symposiasts, his Introduction, and his whole book upon two major themes: inquiry into the brain-behavior interaction, and a description of the techniques used by brain-researchers in the laboratory today.

His next effort lies imbedded in the 13-chapter section on Techniques. This is the kind of paper one might find, if it were somewhat abbreviated, in Glaser's *Medical Physics*. Sheer is concerned with the way one records autonomic responses from animals; measuring blood pressure by attaching a cuff to a man's arm in the doctor's office is easy, but how do you measure blood pressure in a monkey who would rather you didn't? He deals with cardiovascular, respiratory, gastrointestinal and sweat gland methods, at times lightly, at other times with detailed circuit diagrams and actual photographs of the completed hardware ready for action. This chapter, with its practical hints based on long experience, epitomizes the entire section on Techniques. The reader who likes those "How-to-do-it" articles in *Popular Mechanics* will feel at home with these. When not long ago I had a letter from an Australian physiologist asking how one implants electrodes in a cat that is to survive the operation for months, I instantly recommended he buy this book, confident that through studying this techniques section he would find practical answers for most of his questions.

In Sheer's third contribution, "Emotional Facilitation in Learning Situations with Subcortical Stimulation," we find the heart of the book's scientific contribution. I was about to say "the book's contribution to psychology," but this would not be enough. Brain anatomists also will be concerned with the chapters like Sheer's Chapter 31 because his subcortical shocking electrodes invade their territory—the hypothalamus, septal region and amygdala. Brain physiologists too will study Chapter 31 because they have for years placed electrodes in exactly those same brain sites, passed electrical current through them, and collected data upon the behavior evoked. But what Sheer does that these others do not always do is to apply sophisticated behavioral methods to his analysis of the response. Like all the rest of us, psychologists or not, he invades the territory of some other discipline and begs or borrows there whatever knowledge, ideas and techniques he needs to get his work done. But in return he freely offers to one and all the psychologist's unique contribution to brain-behavior research: quantitative measurement of behavioral responses. Sheer (and nearly all the other psychologists who write in this book) does not just look at the behavior; he measures it. Perhaps that's how the APA decides whether someone is bona fide; if he measures behavior he's in, if not, out.

Now let me get off Sheer's back, so to speak, and spend some time on the other 54 participants in the symposium. Their papers are spread through six major sections covering, besides *Techniques* (which I have already disposed of), *Neuroanatomy, Neurophysiology, Behavior, Experiential Correlates,* and a *Summary*. Each section contains several very fine papers, some of which, incidentally, seem to have been prepared long after the Symposium was over. The 200 pages devoted to neurophysiology present 15 papers in all. The spectrum they cover is as wide as the nervous system itself. They forcefully demonstrate the spectacular progress the discoveries of the late 40's made possible. These papers could not have been written five years before the Symposium but not one of them, interestingly enough, seems impossibly out of date five years later. As part of the historical record of the explosion that took place in brain research in the 50's, they will always have a useful place.

Perhaps next in interest are the five papers on *Experiential Correlates*. This funny title covers material of special interest to psychologists concerned with man, for they describe the responses of patients, unanesthetized, to brain shocks. The neurosurgeon who collects basic information on the human brain as he proceeds with his necessarily destructive tasks must move with caution; no clear line divides what he may do from what he may not. Perhaps the reader will find this problem intruding itself as the neurosurgeons and their allied psychologists unfold the story of their successes and failures.

In the *Summary* section a trio of bona fides, Neal Miller, Pribram and Jasper (not many psychologists know that Jasper has a PhD in Psychology from Iowa) discuss implications of the new data for "Theories of Reinforcement," "Systematic Studies of Behavior," and "Neurological Sciences," respectively. Each of these chapters is a thoughtful contribution. Jasper limits himself closely to matters raised in the book; Pribram ranges far more widely; Miller chooses to introduce new experimental material. This reviewer, struggling to discover how best to present Sheer's book, marvels at the way these three, each in his own way, performs the same task.

I CANNOT resist making one more try; an impressionistic review-within-a-review. Not quite a handbook, nor yet merely a collection of loosely related papers. Cook-book descriptions of specific procedures that won't interest more than 50 people in the entire world; broad discussions of theoretical issues. Highly polished contributions from skillful old pros; dull, tarnished, or novice-tinged writing here and there. Would you like to know what happens when electric currents flow through particular portions of the human brain? Your answer is here, complete with pictures of a pretty girl and a verbatim account of her conversation. Like to see some X-rays of a chicken's skull? Maybe you need to know whether copper wire is as good as any other for implanting in a

rat; those vivid photomicrographs of brain harvested one week after silver and copper had been placed within it will sell you on stainless steel, I'm sure. Trivia. Opinions. Facts. Yet for psychology

> "The implications of these neuropsychological efforts are, of course, enormous. After a quarter century of relative quiescence, neurobehavioral studies are again *the* avenue that can lead both psychologists and neurophysiologists to sensible choices among disparate alternatives produced by the prodigious stores of data. The ideational drought that extreme positivism and classical behaviorism imposed on systematic studies of behavior is over. The excesses of a romantic approach to the mind-brain problem have withered during that drought—so it was not in vain" (p. 574).

If you remember yourself as a graduate student—hungry, curious, thrilled by the discovery of neat new facts and great ideas—you may just fleetingly recapture that excitement while reading some part of the book. But even if you are blasé, jaded, over-worked and inclined to consider your youth misspent, you should still remember this book. Perhaps even keep it somewhere near at hand. It is exactly the thing to recommend to a bright, inquisitive graduate student hungry for information about behavior and the brain.

THOMAS S. SZASZ

Muddles, Myths and Medicine

Thomas S. Szasz

The Myth of Mental Illness: Foundation of a Theory of Personal Conduct. New York: Harper, 1961. Pp. xiv + 337. $7.50.

Reviewed by GEORGE A. KELLY

The author, Thomas S. Szasz, was born in Budapest in 1920 and came to this country at the age of 18 to receive all of his higher education here, and to remain. He received his MD from the University of Cincinnati School of Medicine in 1944 and was certificated in psychiatry in 1951. He is now Professor of Psychiatry at the Upstate Medical Center of the State University of New York. The reviewer, George Kelly, has an improbable educational history involving the achieving of a Bachelor's Degree from Park College in 1926, a Master's Degree from the University of Kansas in 1928, a BEd degree from the University of Edinburgh in 1930 and a PhD in psychology from the State University of Iowa in 1931. Before World War II, he taught at Ft. Hays State College, in Kansas, and from there sent an unusual number of his students out into the non-Kansas world for graduate work in psychology. After World War II, during which he served as an aviation psychologist, he spent a brief period at the University of Maryland, before moving in 1946, to a professorship in psychology at the Ohio State University. During his first 25 years as a psychologist he did not publish a great deal; but when he did burst into print it was in a very big way indeed. His two volume work The Psychology of Personality Constructs *appeared in 1955, and is regarded by those who know him, and by many that do not, as a monumental contribution.*

READERS of the *American Psychologist,* which includes most of those who read *CP,* already have an inkling of what Doctor Szasz is up to. His fellow psychiatrists are probably less well informed, and it is a fair guess they will choose to remain so. No one, not even as scholarly a psychoanalyst as the author of this book, can expect to challenge the basic premises of our flourishing psychiatric economy and still manage to collect royalties from his industrious, privately enterprising colleagues. Indeed it is safe to say that this is one dust jacket that will not be used to impress the fifty-dollar-an-hour customers.

Let us admit at the outset that this book did not turn out quite the way your reviewer expected. For one thing, while the author quotes extensively from the logical positivists, he has not actually abandoned the thinking of the nineteenth century dualists. To him what is describable physiologically is essentially physiological, and what can be seen psychologically is itself psychological. For another, he does not question the concept of illness; that remains a venture of bolder minds. He simply points out that what is called "mental illness" is not truly illness, or else that physical and psychological languages are too often confounded in our discourses on this topic.

The story Doctor Szasz tells has its setting in the late nineteenth century. The two principal characters are the imaginative but clinically inept Prof. Jean Martin Charcot, physician at Paris's famed Salpêtrière, and his impressionable student, Dr. Sigmund

Freud, who turns out to be equally imaginative. The plot unfolds out of the medical sociology of the times. That sociology demanded, above all else, that a clear-cut distinction be drawn between illness and malingering. If it was malingering, even out of utmost desperation, the doctor had no business trying to be helpful; but illness—that was something to challenge his most humanitarian impulses! Moreover, to make sure no nonsense slipped in, it was important to classify the illness in terms of disease categories. While this was not too difficult for anyone with a smattering of Greek or Latin, to fail to find a name was to be guilty of malpractice, just as it is today.

In this climate of opinion Charcot was by temperament and opportunity a man of the times. His interests were academic rather than clinical and he did love to classify things. Besides, he was the unchallenged Head Man at the Salpêtrière and no one dared question his morning's pronouncements. His assistants readily learned to select and work up his demonstration cases to fit his favorite diagnostic categories. In no time at all he had added "hysteria" to the lexicon of disease entities, the notion of a nosology of mental disease caught fire, wide-eyed young Freud was more than convinced, and the couch-side search for psychopathodynamics in the twilight zones of the mind was on.

In this headlong enterprise there was a lot of confusion, and Doctor Szasz points it out in great detail—great detail indeed. Surely very little that was ever misstated by Charcot or Freud has escaped his semantic eye. The conversion hysteric, he notes, "had a physical disorder"—and she "didn't have a physical disorder." The anxiety hysteric "had a symbol only"—but he "had the real thing too." Causes got mixed up with purposes, symptoms played hide-and-seek with pathologies, and by the time Sullivan rediscovered the lost ego the whole discipline of psychiatry had become a jumble of physical, psychological, and sociological jargon. All of this was a great disservice to psychiatry which, with its tacit claim to the humanitarian traditions of medicine, deserved a better fate.

The author makes a convincing case. In fact, he makes his case so well that before one is through the first section of the book he is amazed that any two people could be as incoherent as Charcot and Freud. By the end of the indictment, however, he is likely to find himself rooting for these two semantic illiterates who blundered onto some of the most exciting finds of our times.

Hereabouts lies the reason for this reviewer's faint feelings of disappointment. He would like to go all-out in support of a treatise that debunks mental illness—both "mental" and "illness." But the author accepts both constructs. His thesis is merely that they do not go together, that what is mental cannot be illness, and what is illness cannot be mental. But why make Charcot and Freud the scapegoats? The villainy is not the imperfect logic of these two early pioneers—pioneers always have trouble saying what they mean while language remains the ever-potent tool of conventional men. The real villainy is in the cultism that has grown up in the name of Freud, psychiatry, and medicine and which resists by every means, fair and foul, all efforts to inquire into its premises. Why blame Charcot and Freud for that!

But perhaps we are paying too much attention to Szasz's characters and not enough to his plot. He says that nineteenth century sociology expected doctors to separate malingerers from those who were genuinely ill. But what about the distress that leads to malingering? That is genuine too. Charcot and Freud saw malfunction in this kind of feeling, and, wisely or not, they choose to call it a form of illness. Otherwise, how could they, while remaining faithful to their role as physicians, afford to be caught treating persons so afflicted? The sociology of the twentieth century is not much different in this respect. Psychiatrists still think they are, or should be, doctors.

But if Charcot and Freud were trapped by the sociology of their times, how much more were their patients forced to make concessions to contemporary ontology and to the subject-predicate relationship in the language of deterministic materialism. Could a person point to his experience and say that was what hurt? Certainly not! To be taken seriously he must point to the thing that causes the experience. In communicating he must use the words that stand for objects, not the cry that arises out of his own personal existence. This is the game; this is the language. The rules are hard, fast, and "scientifically" enforced. The patients of a generation ago stuck to the rules as best they could. Charcot and Freud also stuck to the rules. Patients developed game strategies and skills. So did Charcot and Freud.

And now the Game of Doctor and Patient has become one of America's most popular pastimes. The price of admission is high and a lot of people cannot afford to participate as much as they would like. But liberal government subsidies will eventually give everyone who wants to play a chance to join in. It should be fun. Some say it is even more fun than sex, but that is hard to believe.

But to come back to Szasz, who does not actually carry things as far as the preceding paragraph suggests. The major part of his book has to do with models which may be used to replace the illness model in dealing with problems psychologically. First, there is language. We must distinguish protolanguage, a pre-logical language form, from language proper, which includes object language and metalanguage. Protolanguage has to be understood in terms of semiotics. In dealing with hysteria, for example, it is particularly useful to be aware of a particular class of semiotics —iconic signs.

Here the author sets a task for himself. Having agreed with Susanne K. Langer that semantic analysis need not stop where discursive language leaves off he points out that "her work in this regard remained mostly programmatic." He says, "One of the purposes of this book is to implement this program by providing a *systematic semiotical analysis of a language-form hitherto regarded as purely expressive*, i.e., *the language of certain bodily signs.*" (The italics are Szasz's.)

Such an analysis gives a clue to the nature of psychotherapy and why "talk-

ing often helps people to cope with their problems in living." He says, "The transformation (for it cannot properly be called translation) of presentational symbols in conventional signs (ordinary words), such as occurs in the course of psychoanalysis and some forms of psychotherapy, is thus seen as itself constituting a process of personality change ('treatment')." Nor does the author stop with an analysis of the psychotherapeutic process; he is concerned with personality change in general, and he extends his semiotic analysis to the development of the child through different language stages.

In the last two parts of his book the author sketches the theory of games, from the casuistic rule-following systems of the moralists to the logical games that mathematicians and scientists play. Having done so, he comes back to hysteria, the only disorder he discusses extensively in his book, and shows how it conforms to the game model.

In conclusion he points out that there is "no transcendentally valid (God-given) game," an insight that often leads Western man to the brink of despair. Yet man can choose "to rise to the challenge of the unceasing need to learn and relearn . . . Some require a personal instructor for this; others do not."

Exeunt: Priest and Doctor
Enter: Philosopher and Educator

Political Psychologist at Work

Erich Fromm

May Man Prevail? An Inquiry into the Facts and Fictions of Foreign Policy. Garden City, N. Y.: Doubleday, 1961. Pp. v + 252. $4.50.

Reviewed by RAYMOND A. BAUER

Erich Fromm is a very well-known German trained, psychoanalytically oriented psychologist who lived and worked and wrote in this country for a number of years before moving, in 1951, to a professorship at the University of Mexico. Perhaps the best indication of his stature is the fact CP *reviews not only his own books* (CP, *Sep., 1956,* **1,** *271-2), but reviews books written about his books* (CP, *June, 1962,* **7,** *225). The reviewer, Raymond Bauer, received his PhD in the Department of Social Relations at Harvard University, and after some years a mile or so down the Charles River at the Massachusetts Institute of Technology, crossed over the river to the Harvard School of Business Administration. A social psychologist with a special interest in public and political problems, he has become a specialist in the study of the Soviet Union and is author or co-author of a number of books dealing with Russia, among them* The New Man in Soviet Psychology (1952) *and, with Alex Inkeles,* The Soviet Citizen: Life in a Totalitarian Society (CP, *July, 1960,* **5,** *218).*

THIS is an informed, thoughtful, serious, even passionate discussion of major problems facing the world today, written by a renowned and articulate psychoanalyst and political psychologist. The title of the substantive chapters indicate the ground covered: "The Nature of the Soviet System"; "Is World Domination the Aim of the Soviet Union?"; "The Meaning and Function of Communist Ideology"; "The Chinese Problem"; "The German Problem"; and "Suggestions for Peace."

In discussing Soviet and Chinese Communism, Fromm relies on secondary sources. Where the reviewer feels on surest ground, i.e., the Soviet field, he found Fromm's choice of sources to be very good, and the interpretations of events to be generally informed and well reasoned. The sources on China are very probably also good, and I assume the others are equally reliable. When dealing with the German problem, Fromm appears to rely primarily on his own expertise and on current journalistic reporting. Here the reader might wish to check the strength of Fromm's anti-German feelings against other reliable sources. From my own reading of the press, the evidence on the role of former Nazis in the Adenauer regime is by no means as clearcut as Fromm maintains.

In the course of arriving at his suggestions for peace, Fromm reviews many of the problems and arguments associated with a strategy of deterrence, arms control, and various forms of disarmament. The treatment is generally informative and temperate. The exception is a rather choleric approach to Herman Kahn's suggestion that there may be circumstances under which viable social life might exist after a nuclear attack. Fromm concludes that the only condition under which we can have stable peace is with universal controlled disarmament. This policy should be accompanied by fostering of democratic socialism in under-developed countries. Fromm observes realistically that there would be difficulties in getting such policies supported by Americans. This is because Americans are lacking in proper goals. Thus, one may agree with him on the desirability of universal disarmament, and on the difficulty of attaining that state, but still retain some reservations concerning such sentiments as: "We are developing into faceless organization men, alienated from ourselves and lacking authentic feelings and convictions." "What can save us and what can help mankind is a renaissance of the spirit of humanism, or individualism, and of American's anti-colonialist tradition."

This is not to suggest that any of us would be against humanism, or individualism, or anti-colonialism, but rather that we may wonder if salvation is to be found in a humanistic camp meeting.

When Fromm suggests universal controlled disarmament, let the reader be

assured that he does not do this from naiveté about either Soviet or Chinese communism. This reviewer has points on which to cavil with Fromm, but in general his analysis of the forces at work in the communist world is at the very least as competent as that which is ordinarily available, and much better than most.

In short the intelligent reader who wants an informed run-through of these important topics, together with stimulating, but controversial analysis will find this book worthwhile reading. He will be grateful to Fromm for avoiding and in fact challenging many popular clichés. This reviewer hopes, however, that the reader will be aware of the fact that Fromm has his own clichés.

ALL of the above can be said with comfort and ease, but the remainder of the review comes with considerable difficulty. With Fromm having covered so much ground, it would be a rare reviewer who would not differ with him on many specific points. But, this can lead to irresponsible academic gun play. Let us accept the fact that there are many specific points on which Fromm could be challenged. With this accepted, the intent of the reviewer is then to encourage the reading of this book, but to convey to the reader a sense of critical awareness which will make him more than a passive digestor of Fromm's ideas.

Fromm's psychological background is generally an advantage. When he deals with the problem of ideology, he shows detachment and understanding of how people can say things which do not square with the reality beneath their nose and still not be "lying" in a moralistic sense. His discussion of the various modes of perceptual distortion which interfere with the American view of Soviet reality is also a contribution. His *description* of what he calls "paranoid" thinking is authentic. Americans do over-rationalize and over-interpret Soviet behavior. Soviet observers reciprocate. However, the connotation of individual pathology associated with the term "paranoid" is misleading. A social psychological and functional analysis of this type of misperception—beyond the scope of this review—could remove the aura of personality pathology associated with the term "paranoid" and point to some more constructive course of action than the moral-muscular tugging at one's boot-straps that is recommended in the terminal pages of the book.

It has been suggested that Fromm has his own set of clichés. These can be located if the reader of the book will check the text for non-sequitors. Thus, Fromm has a deep commitment to the notion that Americans have lost their sense of values and purpose. Be this right or wrong, he drags it in by the heels from time to time. For example, when he reviews Herman Kahn's discussion of the practical possibilities of a viable atomic war, he goes out of his way to point out that even Kahn's associates in the Rand Corporation apparently do not agree with him. But, a few pages later, we find the following sentence: "That this kind of thinking has become so popular is one of the gravest symptoms of despair and alienation . . ." Where did Fromm find evidence that Kahn's position was popular? Fromm himself said it was unpopular. Apparently he could not resist the opportunity to get in a dig at "despair and alienation."

WE PSYCHOLOGISTS would like to pride ourselves in rationality. But, a sure sign of the limits of rationality is the existence of preferred solutions. We like some answers better than others. Thus, Fromm closes out his chapter on China with this statement: ". . . the more difficult economically the Chinese position is, the more aggressive its foreign policy. If the present humiliation of China is continued, the aggressive tendencies within China will increase and help Khrushchev's enemies within the Soviet Union to gain victory." He then goes on to project the thermonuclear arming of China, then Germany, and next the brink of an atomic war unless the Peking government is given credits and the possibility of free trade.

This reviewer has little or nothing against this conclusion, but it does not follow tightly from preceding evidence and argument. If one backs up from this point into the preceding dozen pages several times he will probably find nothing that makes this conclusion inevitable. As a matter of fact it might well be argued that a few years of famine and/or industrial disorganization would bring the Chinese communists to heel. The reviewer does not necessarily advocate this, but must in conscience point it out as a logical possibility.

Fromm directs his words to the general public, and draws a bead on their clichés. Good show; the only good cliché is a dead cliché. But psychologists have their own clichés—about increased conformity in American society, pathological-type distortions in perception, German national character, etc. Since Fromm has done us the service of making us think, let us honor him by continuing to think as freshly as possible even while reading his work.

A Mixture of Tastes

Morley R. Kare and Bruce P. Halpern (Ed.s)

Physiological and Behavioral Aspects of Taste. Chicago: The University of Chicago Press, 1961. Pp. vii + 149. $6.00.

Reviewed by JOHN F. HAHN

The editors of this volume, Morley R. Kare and Bruce P. Halpern, are both professors of physiology, Kare in the Department of Zoology at North Carolina State College and Halpern (although he is a psychologist) in the Department of Physiology in the College of Medicine of the Upstate Medical Center of the State University of New York. Both also have had continuing contacts with psychologists, Dr. Kare principally with R. B. MacLeod at Cornell and Dr. Halpern, since his first days of graduate school, with Carl Pfaffmann at Brown, with whom he did his doctoral work. John F. Hahn, the reviewer, is Associate Professor of Psychology at the University of Virginia. As an under-

graduate he got intellectually caught up in chemistry but switched to psychology because it was more fun. He pursued the fun with Dewey Neff at the University of Chicago, where he received his PhD. At Virginia he has been studying senses of the skin and reports the likelihood that he will continue to study Virginia skins even after Frank Geldard has taken his own off to Princeton.

THE INTERDISCIPLINARY symposium, like the pack rat, can be found throughout much of the United States and often leaves behind some article as a token of its visit. The symposium participants generally find their visit to have been stimulating and enjoyable, but whether the article serving as a memento will be valuable to the bystander depends not only on it but on him.

The slim volume which reports the proceedings at the 1960 Cornell symposium on physiological and behavioral aspects of taste is likely to be of value to a reader only if he meets certain requirements. He should be interested in research on taste, ingestion, metabolism, and their relations to one another. He should not be looking for a systematic survey of the literature, for it is unreasonable to expect this of a symposium. He should not expect to learn of important improvements in techniques, or of significant new insights, for the participants had none to report. Nor should he be a reader of general interests who hopes for a systematic, non-technical exposition. The participants, quite naturally, write for each other rather than for him, concentrating on their current research and tending to assume that everybody knows about *nucleus ventralis*, RC time constants, or reduced triphosphopyridine nucleotide.

If the reader's own research is in the area of taste, ingestion, metabolism, or their interactions, he may expect to obtain some of the benefits he would have if he had attended the symposium. Because it is difficult to keep up with all the work in all the areas which might conceivably have some bearing on one's own research, there is always the possibility that hearing from one's colleagues in these areas may supply one with a useful piece of information. Moreover, although a symposium inevitably catches a few participants between findings, the chief justification of an interdisciplinary affair such as this is not merely in the factual content *per se* but in the hope that each participant will come away with new perspectives and new ideas. The same possibility exists for the reader.

THE PARTICIPANTS are drawn from several disciplines, but are more aptly grouped by their approaches than by their formal affiliations. Phenomenology of taste is represented by R. B. MacLeod's brief essay, "What is a sense?", and by F. J. Pilgrim's report of how representatives of the conventional four taste qualities affect each other's subjective intensities. Behavioral studies of preference and ingestion in the rat are provided by K. R. Christensen and H. L. Jacobs, with M. R. Kare adding some observations on other species. Sensory physiological contributions consist of R. L. Kitchell's sample neural recordings and methodological comments, plus G. P. Frommer's studies of thalamic responses to taste solutions. Metabolic consequences of some different ingestion patterns are described by Jay Tepperman and F. W. Heggeness.

The papers are preceded by Carl Pfaffman's graceful preface, uniting these diverse contributions as all being relevant to a sense with important cognitive, reinforcing, and life-sustaining properties. They conclude with H. L. Pick's survey of taste research in the Soviet Union, and his impression that the experiments are usually sound but the published accounts sketchy.

There are also seven discussions, a final discussion, an index, and a list of references which contains, in addition to a few general references, 202 literature citations. The journals referred to are from many fields: from psychology and physiology, anatomy and zoology, through genetics, food research, and chemical education, to gerontology, endocrinology, and poultry and dairy science. This array demonstrates why the interdisciplinary symposium will doubtless continue to thrive.

A Little New Light on an Old Profession

Maryse Choisy

Psychoanalysis of the Prostitute. New York: Philosophical Library, 1961. Pp. 138. $4.75.

Reviewed by REUBEN FINE

Maryse Choisy the author, is a French psychologist and psychoanalyst who, among other things, serves as editor of Psyche, *the international journal of psychoanalysis. A number of years ago she wrote* A Month Among the Girls, *a book also dealing with the life of prostitutes. The reviewer, Reuben Fine, is a psychoanalytically trained and oriented psychologist, who is now engaged in private practice and who serves as supervisor of psychotherapy at the Elmhurst, L. I. General Hospital. He is a leading figure in the National Psychological Association for Psychoanalysis and is both a proponent and forwarder of the training of non-medical analysts. Due to be available in the Fall of (1962) is his book* Freud: A Critical Reevaluation, *to be published by the David McKay Company.*

BY NOW the literature devoted to the oldest profession in the world is quite voluminous. The prostitute has been described and discussed from every conceivable point of view—sexual, economic, religious, aesthetic, moral, amoral and immoral.

Psychoanalysis has added a dimension of depth to previous investigations. No doubt the best study so far is the well-known book by Harold Greenwald: *The Call Girl.* It has been reasonably well accepted that, at least as far as Western culture is concerned, the prostitute is a girl who lacked love in childhood, feels rejected and mistreated by the father, and accordingly remains frigid and seeks revenge against men. The sex act between the prostitute and her customer has been aptly described as a union between an impotent man and a frigid woman.

Any clinical psychologist will recognize immediately that this situation is not at all unusual or rare. Maternal rejection has become to the psychotherapist what the germ theory of disease was to the medical researcher of the early 1900's. And ever since Freud's pioneer labors, the widespread existence of sexual difficulties in both men and women has been a matter of common knowledge. In fact, Kinsey, in spite of his anti-Freudian bent, tends to confirm the psychoanalytic position in many particulars.

With this general background one turns with two major questions to a book on the psychoanalysis of the prostitute. First, what distinguishes the promiscuous girl, who submits to men for pleasure, from the girl who does it for money? And second, what is there in the childhood experience of the prostitute that has led her to turn to it, rather than merely fantasize about it, as so many women do?

In both these respects the present book is sadly lacking. A number of generalizations, which can be found anywhere, are cooked together with a few superficial case histories. The result is a meal of no distinction.

Two case histories of prostitutes are both inadequate. One is unusual, in that the girl gave up the "racket" to become a nun; but the dynamics of this sudden change are not made clear.

In this particular area as in so many others, we are confronted with a large variety of information which need better organization and elucidation. To this end it is necessary to ask specific questions that will help to order the material. Hypotheses can readily be constructed to foster more fruitful research. For the prostitute we could say, in psychoanalytic language: in the light of the fact that so many girls have prostitution fantasies, how is it that the superego of one permits the activity, while that of another forbids it? Psychoanalytic research, with its access to otherwise inaccessible clinical detail, could open the road to some answers. Choisy's book sheds some light on the actual psychoanalyses of two prostitutes but make no contribution to the larger question.

One Clinician's Practices

Molly Harrower

The Practice of Clinical Psychology. Springfield, Illinois: Charles C Thomas, 1961. Pp. xiv + 321. $12.00.

Reviewed by HOWARD L. SIPLE

Molly Harrower, since receiving her doctorate at Smith College in 1934, has had a rich and varied experience as clinical practitioner, as research worker and as teacher, and concurrently, has raised and educated a family, has written poetry and has kept up her proficiency at the game of golf. She now is on the point of disengaging herself from full time private practice to conduct follow-up studies of two thousand or more cases upon whom she has in the past conducted diagnostic studies. Howard Siple, the reviewer, received his PhD at Northwestern University, where his graduate training was interrupted by a period of service as a clinical psychologist during World War II. He taught for a while at the University of Rochester and then, for the past twelve years, has been a psychologist with the Veterans' Administration. He has held positions at the Topeka Hospital, at the Denver Hospital, at the Mental Hygiene Clinic in Buffalo and most recently, as chief clinical psychologist in the Mental Health Clinic, Veterans' Administration Regional Office, San Francisco. Throughout his career with the Veterans' Administration, he has maintained a broad pattern of involvement with psychology: he has held teaching appointments at the Menninger School, at the University of Kansas, at the University of Colorado and at the University of Buffalo and since 1952 has had a small private practice. He committed himself in Buffalo to prepare the present review but it was posted in San Francisco.

IN THE "good old days" when almost all psychologists were scientists and communicators of science, there was little concern about roles, functions, ethics, supervision and a host of other distractions. There was also little, if any, private practice. Then the world began to beat a pathway to our professional door and a large number of us became "doers" as well as "knowers." Clinical psychologists found themselves in unfamiliar institutional structures where they tried to make themselves useful in state hospitals, industrial settings, VA hospitals and clinics, community clinics, medical schools, etc. Many of them, first flattered and excited by their new tasks, gradually became disillusioned with work in settings structured primarily by non-psychologists. Kelly and Goldberg (1959) found 40% of the clinical trainees in their early assessment project wishing, ten years after starting their professional training, that they had chosen another related occupation. Some of the most venturous souls did courageously "cast off the shackles" of the institutional settings. Many more went into part-time private practice, perhaps hoping for a modicum of freedom and self determination without relinquishing their secure, salaried positions. Now, in 1962, private practice has become at least part of the way of life of a large number of psychologists, even though a social, professional and scientific model of the private practitioner remains to be defined.

Dr. Harrower has long been "among those attempting to define and develop the role of the independent clinical practitioner." Her book is a collection of exhortations, convictions, warnings, case studies, lectures and techniques coming out of the process. If the reader

keeps this clearly in mind, he may not be disturbed by the fact that about one-fifth of the book is devoted to the unedited answers of some seventy colleagues, mostly in the New York City area, to a questionnaire about their individual practices. He will see these answers for what they are, the practices of people known to the author, not a random sampling of people in private practice either in New York City or in the country as a whole. The reader will also be aware that these practices are not a model of what private practice should be.

WHAT does the book offer the young psychologist considering private practice? Not as much as the author hopes, unfortunately. It suggests a few grossly oversimplified "Do's and Don'ts": DO have enough cash to see you through a year of getting established; DO know that the community needs and wants the services you have to offer; DO be known in your own profession; DO know some of the medical profession; DO proceed with caution; DO establish a procedure for accepting cases; DO decide on a core battery of tests; DO be aware of the problems inherent in the growth of a new profession; DON'T go directly into private practice; DON'T run counter to established professional mores; DON'T be naive about money; DON'T be a lone wolf. The reviewer has no objection to such platitudes, but do they give the young psychologist a sound basis for making vocational decisions? Is advice going to give the profession of psychology a group of practitioners who can and do apply the scientific knowledge to human problems? It would be easier to recommend a book to the person considering private practice if it posed crucial problems *in* the practice of clinical psychology rather than listing do's and don'ts *about* it.

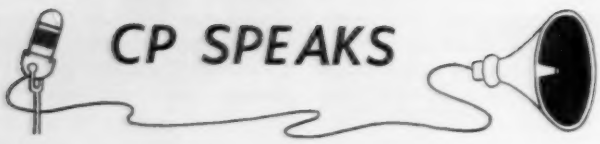
The group of readers who will find this book most interesting and useful may well be those who are established in private practice, those who want to compare what they are doing with what Dr. Harrower has done and their working assumptions with hers. For those who like to take a stand either with or against another clinician on a variety of controversial issues, Dr. Harrower has knowingly and courageously "stuck her neck out." You can compare a speech you have given to a PTA group with the one she has given. You can ask yourself what you would say if asked to talk on mental health to a group of permanently handicapped, bed-ridden patients; then compare with the lectures she gave. Making these comparisons, as well as comparisons with the author's clinical judgments and actions was fun for the reviewer. However, he found himself comparing *ad hominem* rather than *ad verbum* at times.

The book also contains a number of interesting devices for the mature practitioner to consider adopting if he has not yet tried them. For example, there is the idea of asking a referring therapist to rate a patient on the same set of characteristics the diagnostician rated from test findings; this permits a sort of "clinical validation." Another device: ". . . including parallel Rorschach responses, along with a running commentary on the responses, can be made use of not only for one individual at different stages of life, but also when there are two persons involved and a comparison between them is desired." The author describes a number of procedures, one of the most startling of which is the technique of using a notebook for communication between therapist and patient during prolonged absences of the therapist. The patient writes "whatever is on his mind" on the right hand page, mails the notebook to the therapist who responds with comments or interpretations on the left hand page and mails the copybook back to the patient.

A THIRD group to whom the book is addressed, and one that will probably be most negatively critical, consist of the psychologists who want to discourage the "over eager student." The whole tone of the book will probably do little to evoke reactions of caution, doubt and deliberation. It makes almost everything a little too easy by provoking agreement or disagreement. It does not ask the would-be practitioner to think seriously about the fact that research has not yet given us a sound basis for most of the things we do in private practice. It does not tell us how and in what kinds of problems our personality theories are more or less adequate for understanding the range of human problems which comes to us. It does not ask us why, when we attempt psychotherapy, we fall back more readily on our basic "humanness" than on a firm body of scientific knowledge, nor what we should do about it. It does not make us wonder why many of our colleages will reject us as psychologists if we enter private practice as "doers." Psychologists who want to discourage "over eager students" might do much better by giving them a few carefully selected research reports on clinical topics than by referring them to *The Practice of Clinical Psychology*.

Despite the wide scope of problems and examples Dr. Harrower selected from her private practice for inclusion in this book, there are three rather glaring omissions. There is nothing at all about future prospects in clinical practice. In this connection some attention to the Joint Commission reports with special attention to the implications for private practice might have been valuable. Redefinitions of psychological practice in the newer certification and licensing laws may also have quite an effect on future practices. Second, there is nothing in this book about group therapy, an activity in which quite a number of practicing psychologists are becoming increasingly involved. Third, there are many special opportunities and problems in the relationships between the private practice of psychology and organized religious groups. This area is largely neglected, not only in this book but in the writings of other practitioners.

Richard Blanton recently wrote, "The definition of the role model for clinical psychologist has been a matter of dispute from the beginning." The private practice of clinical psychology remains a highly personal business. Dr. Harrower's book is a courageous demonstration of how one clinician went about her pioneering business. Perhaps the next fifteen years will bring better basic theory, more unification of practice and a less disputed definition of the practitioner's role. Then it may be easier for someone to write better books than Blau's (*CP*, March, 1961 **6**, 74-75) and Harrower's.



Books to Come

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Also coming before Christmas, from Macmillan, is Michael Hirt's *Rorschach Science: Readings in Theory and Method*. Dr. Hirt says his selection of readings will not have the cook-book emphasis but will deal primarily with evaluations of Rorschach's Test as a measuring instrument. He wants to help save the Rorschach procedure from stagnation by contributing to the development of a healthy, repeat healthy, skepticism about it.

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"A collection of books under the general title, "Le Psychologue" has been appearing since 1957, edited by Paul Fraisse, Professor at the Sorbonne and Director at the École Pratique des Hautes Études, and published by the Presses Universitaires de France. The collection is intended eventually to cover all of the areas of psychology, both academic and applied. The series is meant both for undergraduate students and also for people from other fields—such as industry, medicine, and social work—who may wish to collaborate with psychologists and who therefore need to bring themselves up to date on psychological questions. Each book—a small, paperbound volume—treats a single subject or area. The editor requests each author to present a clear and well digested synthesis of the material, purposely omitting extensive bibliographic references.

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9. Didier Anzieu. Les methodes projectives.
10. Georges Bastin. Les techniques sociometriques.
11. Rémy Chauvin. Le comportement social chez les animaux.
12. Jean-François Le Ny. Le conditionnement. (See review following.)"

Now, out of France and back to Russia for a moment. Daniel E. Berlyne, who is an informed observer of international developments in psychology and who has recently visited a number of Russian institutes and laboratories, makes the following optimistic observation about increasing access to the work of Russian psychologists.

"Channels by which accurate and up-to-date information about Soviet psychology can be obtained are opening up rapidly. More and more Western psychologists are making the discovery that psychological and physiological Russian, being about as limited in style and vocabulary as its English counterpart, is not so difficult to master. Those who are restricted to English can benefit from the erudition of men like Razran, London, and Mintz, through the review articles that they provide from time to time. Even these secondary sources, valuable as they have been, will soon be unnecessary, as some of the principal Russian journals of psychological interest are coming out in English versions, and most of the important books that have been published in the Soviet Union within the last few years are now in the process of being translated."

—F. H. S.

Gallic Clarity on CR's

Jean-François Le Ny

Le Conditionnement. Paris, France: Presses Universitaires de France, 1961. Pp. 172. 8 NF.

Reviewed by MARK R. ROSENZWEIG

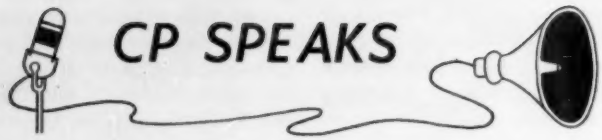
Jean-François Le Ny, the author, has been identified by the reviewer, who is Mark R. Rosenzweig, now Professor of Psychology at the University of California, Berkeley. Rosenzweig received his MA at the University of Rochester and his PhD from Harvard. He has been at Berkeley since 1950, except for two sabbatical semesters, one in 1955 and another in 1960, both spent at Fraisse's laboratory at the Sorbonne. Here, while doing research on psycholinguistics, in French, he became acquainted with Le Ny. His research since 1954 has focused on brain mechanisms in learning and his publications, with his colleagues at Berkeley, have shown that training can produce measurable changes in both brain chemistry and brain anatomy. He has taken time out, now thrice, from his work on the brain, in order to use his own in doing reviews for CP. *On Page 370f. of the present issue he makes yet another contribution.*

AN ACTIVE interest in conditioning has been developing only rather recently in France, in spite of the early publicity that was given to Pavlov's work by Henri Piéron and in spite of the discovery of conditioning of the EEG by Durup and Fessard in Piéron's laboratory in 1935. In 1955 a symposium on EEG studies of conditioning was held at Marseilles, and the resulting publication was reviewed in *CP* in 1958 (pp. 218-219). Then the Association de Psychologie Scientifique de Langue Française held a symposium on conditioning and learning in 1956, the resulting publication being mentioned in *CP* (1959, p. 317). Now the present book is an attempt to inform French undergraduates, as well as interested readers from other fields, about the basic findings and current status of research in conditioning. At the same time it is an attempt to foster interest in the field. As in the other volumes in the series, "Le Psychologue," (see CP SPEAKS, p. 000) the author of *Le conditionnement* has sought to be clear and concise rather than complete.

Dr. Le Ny has an extensive background in research on conditioning. His doctoral thesis at the Sorbonne was on stimulus and response generalization, and he has published several articles on human conditioning and verbal behavior. Recently he spent a period at Konorski's laboratory in Warsaw, a productive center of research on animal conditioning. Dr. Le Ny now instructs at the University of Lille where he gives a course on conditioning.

The range of subject matter included in *Le conditionnement* is almost the same as that covered in Kimble's 1961 revision of Hilgard and Marquis' *Conditioning and Learning*. Only two chapters of Kimble's text—"Conditioning in Historical Perspective" and "Applications to Personality"—are not paralleled by material in Le Ny's book. Furthermore, the French book contains a final chapter on higher processes, treating chiefly voluntary behavior and language, which is not paralleled in the American text. In both books the concentration is on behavior, with little mention being made of neurophysiological studies of conditioning. The treatment in *Le conditionnement* is rather brief throughout, its text being only about a quarter the length of Kimble's.

SINCE bibliographies drawn up by American authors are often accused, with reason, of being parochial, it is interesting to see what sources are cited by a European author. In the preface,



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7. Charles Chandessais. La psychologie dans l'armée.
8. Suzanne Pacaud. La sélection professionelle. (Reviewed in *CP*, July, 1962)
9. Didier Anzieu. Les methodes projectives.
10. Georges Bastin. Les techniques sociometriques.
11. Rémy Chauvin. Le comportement social chez les animaux.
12. Jean-François Le Ny. Le conditionnement. (See review following.)"

Now, out of France and back to Russia for a moment. Daniel E. Berlyne, who is an informed observer of international developments in psychology and who has recently visited a number of Russian institutes and laboratories, makes the following optimistic observation about increasing access to the work of Russian psychologists.

"Channels by which accurate and up-to-date information about Soviet psychology can be obtained are opening up rapidly. More and more Western psychologists are making the discovery that psychological and physiological Russian, being about as limited in style and vocabulary as its English counterpart, is not so difficult to master. Those who are restricted to English can benefit from the erudition of men like Razran, London, and Mintz, through the review articles that they provide from time to time. Even these secondary sources, valuable as they have been, will soon be unnecessary, as some of the principal Russian journals of psychological interest are coming out in English versions, and most of the important books that have been published in the Soviet Union within the last few years are now in the process of being translated."

—F. H. S.

Gallic Clarity on CR's

Jean-François Le Ny

Le Conditionnement. Paris, France: Presses Universitaires de France, 1961. Pp. 172. 8 NF.

Reviewed by MARK R. ROSENZWEIG

Jean-François Le Ny, the author, has been identified by the reviewer, who is Mark R. Rosenzweig, now Professor of Psychology at the University of California, Berkeley. Rosenzweig received his MA at the University of Rochester and his PhD from Harvard. He has been at Berkeley since 1950, except for two sabbatical semesters, one in 1955 and another in 1960, both spent at Fraisse's laboratory at the Sorbonne. Here, while doing research on psycholinguistics, in French, he became acquainted with Le Ny. His research since 1954 has focused on brain mechanisms in learning and his publications, with his colleagues at Berkeley, have shown that training can produce measurable changes in both brain chemistry and brain anatomy. He has taken time out, now thrice, from his work on the brain, in order to use his own in doing reviews for CP. *On Page 370f. of the present issue he makes yet another contribution.*

AN ACTIVE interest in conditioning has been developing only rather recently in France, in spite of the early publicity that was given to Pavlov's work by Henri Piéron and in spite of the discovery of conditioning of the EEG by Durup and Fessard in Piéron's laboratory in 1935. In 1955 a symposium on EEG studies of conditioning was held at Marseilles, and the resulting publication was reviewed in *CP* in 1958 (pp. 218-219). Then the Association de Psychologie Scientifique de Langue Française held a symposium on conditioning and learning in 1956, the resulting publication being mentioned in *CP* (1959, p. 317). Now the present book is an attempt to inform French undergraduates, as well as interested readers from other fields, about the basic findings and current status of research in conditioning. At the same time it is an attempt to foster interest in the field. As in the other volumes in the series, "Le Psychologue," (see CP SPEAKS, p. 000) the author of *Le conditionnement* has sought to be clear and concise rather than complete.

Dr. Le Ny has an extensive background in research on conditioning. His doctoral thesis at the Sorbonne was on stimulus and response generalization, and he has published several articles on human conditioning and verbal behavior. Recently he spent a period at Konorski's laboratory in Warsaw, a productive center of research on animal conditioning. Dr. Le Ny now instructs at the University of Lille where he gives a course on conditioning.

The range of subject matter included in *Le conditionnement* is almost the same as that covered in Kimble's 1961 revision of Hilgard and Marquis' *Conditioning and Learning*. Only two chapters of Kimble's text—"Conditioning in Historical Perspective" and "Applications to Personality"—are not paralleled by material in Le Ny's book. Furthermore, the French book contains a final chapter on higher processes, treating chiefly voluntary behavior and language, which is not paralleled in the American text. In both books the concentration is on behavior, with little mention being made of neurophysiological studies of conditioning. The treatment in *Le conditionnement* is rather brief throughout, its text being only about a quarter the length of Kimble's.

SINCE bibliographies drawn up by American authors are often accused, with reason, of being parochial, it is interesting to see what sources are cited by a European author. In the preface,

Le Ny states, "We have tried to be correctly informed about what is being done in conditioning in the U.S.S.R., in Poland, and in the United States, which are the great centers of research in this field." About half of the publications cited by Le Ny turn out to be by American authors, while the other half are split about equally between Russian and French authors.

Although Le Ny depends relatively heavily on Russian sources, which he is able to read in the original, his account of conditioning is not restricted to orthodox Pavlovian interpretations. For example, he notes that while the Pavlovian concepts of irradiation and concentration are a possible way of explaining generalization and discrimination, they are not the only way, and in fact they no longer seem to be the most probable way. In general Le Ny prefers not to deal with the physiological concepts of Pavlov, partly because they are of doubtful value and partly because neurophysiology now has available techniques for studying central neural processes directly while still (as Pavlov insisted) preserving the functional integrity of the organism. Furthermore, Le Ny points out that although Pavlov had wanted to plant his structure solidly in the physiological domain, a slippage of scientific boundaries has now moved the structure to—or even across—the frontier of psychology. That is, what has proved valuable and enduring in Pavlov's work has been the objective study of behavior, which has now become the field of psychology.

His knowledge of Russian has helped Le Ny to clarify some problems of translation that have interfered with understanding the Russian authors. Thus, for example, he indicates why "conditional reaction" conveys the originally intended meaning better than "conditional reflex." There is a glossary of almost 100 items which also serves as an index.

Le conditionnement fulfills admirably its author's intent of presenting a clear, concise and impartial introduction to conditioning and its place in behavioral science. Hopefully it will also serve to encourage research in this area among French and French-speaking psychologists.

A Behaviorist Views Behaviorism

D. E. Broadbent

Behaviour: A Survey of 20th Century Theory in Behaviouristic Psychology. New York: Basic Books, 1961. Pp. 215. $4.50.

Reviewed by JOHN P. SEWARD

In the late thirties, D. E. Broadbent started to read engineering at the University of Cambridge but was interrupted by the war and a tour of duty with the RAF, where he became interested in psychology through observing problems of human engineering and personnel selection. He returned to Cambridge to read psychology and took his degree in 1949 with Sir Frederic Bartlett. Ever since his Cambridge days he has worked in the Applied Psychology Research Unit of the Medical Research Council, becoming its Director in 1958. He is the author of Perception and Communication *(Pergamon, 1958). The reviewer, John P. Seward, took his AB in 1926 at Cornell University and left Ithaca untouched by Tichenerian structuralism. He went on to Columbia University to find out what else psychology had to offer. Later he began studying guinea pigs, rats and now cats. He says that his active interest in behavior theory was first stimulated by Hull's* Review *articles in the 30's. He has experimented on latent learning and extinction and on the role of intervening variables in appetitive and aversive learning, with his research combined with his teaching, at Columbia, Connecticut College, Yale University, Boston University, and at the University of California, Los Angeles, where he is now a professor and where he now leans toward the views of Tolman, Woodworth, and Mowrer rather than Hull, and toward Spence's current position rather than the earlier Spence writing.*

IN A FOREWORD Broadbent states part of his purpose in writing *Behaviour*. He is seeking a wider public recognition of the importance, even urgency, of an objective psychology. Apparently psychologists in the United Kingdom have less status—and less support—than their colleagues in the United States. The author therefore addresses the "intelligent layman" in an effort to explain what a science of behavior is about and to share something of its fascination. His goal is evidently education rather than propaganda. In its attainment "some excuse may be found for the applied psychologist who left his proper concerns to write this book."

If that were all, American psychologists might well ask why they should read it. But, as his previous publications reveal (e.g., *Perception and Communication*, 1958), Broadbent is at heart more theoretician than applied psychologist. A second aim, disclosed in a personal communication, stemmed from the "violent antipathy" of his colleagues at Cambridge toward S-R theory, U. S. brand. The author himself is convinced that the S-R approach will ultimately give way to cybernetic concepts. But he does not excuse "the tendency of many psychologists to stand not on the shoulders of their predecessors, but rather on their faces." To counteract this tendency he attempts—and achieves—a balanced appraisal of a largely imported product. American behaviorists now have the unusual opportunity of seeing themselves as one erudite British behaviourist sees them.

The author has focused on the area dominated by such names as Hull, Tolman, Lashley, and Skinner, and has sketched some of the significant developments in that area, between Watson and the present. He has handled the formidable problem of selection with

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By DOROTHY RETHLING SHAFER, University of Florida. McGraw-Hill Series in Psychology. *Available in January 1963.*

Emphasizing changes in human activity, this advanced book uses the experimental approach. It considers a biological approach to motivation, but stresses studies using human subjects. The book is organized around such concepts as arousal, personal pace, drive, and drives. In addition, consideration is given to choice as motivationally determined. Illustrations are drawn from both industry and clinical observations.

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By LEONARD BERKOWITZ, University of Wisconsin. McGraw-Hill Series in Psychology. *384 pages, $7.95.*

Emphasizing the actions of human beings in social settings, this book reviews theoretical and research material dealing with aggressive behavior. Material included ranges from ethological and biological observations, animal experimentation, personality theory and research, social psychological conceptions and research, and relevant sociological formulations. Investigations are summarized and brought together under a common theoretical framework.

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☐ INDIVIDUAL IN SOCIETY: A Textbook of Social Psychology

By DAVID KRECH, RICHARD S. CRUTCHFIELD and EGERTON L. BALLACHEY, all of the University of California, Berkeley. 516 pages, $7.95.

The purpose of this book is to organize fact and theories from general psychology, personality psychology, social psychology, anthropology and sociology, showing the interrelationships which exist among these areas. The four main study areas are: basic psychological factors; attitudes; social and cultural habitat of man, and group; and organizational behavior. A complete revision of Krech and Crutchfield's *THEORY AND PROBLEMS OF SOCIAL PSYCHOLOGY.*

☐ VERBAL BEHAVIOR AND LEARNING: Problems and Processes

Edited by CHARLES N. COFER, New York University; and BARBARA S. MUSGRAVE, Smith College. McGraw-Hill Series in Psychology. *Available in March, 1963.*

This volume represents the proceedings of the Second Conference on Verbal Learning held under the auspices of the Office of Naval Research and New York University in June, 1961. The majority of the contributors, all top scholars, are the same as those which appear in the book developed from the first conference: *VERBAL LEARNING AND VERBAL BEHAVIOR,* McGraw-Hill, 1961. Presents overview of the entire field of verbal learning and its relation to verbal behavior. For the advanced student and professional psychologist.

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such skill that the reviewer wonders if he was aided by distance, as if mapping a confusing terrain from the air. In any event, it is interesting to note which points of inquiry loom largest from across the ocean.

WHAT emerges is an integrated picture of behavior theory developed on two levels: (1) the "tactical" level, where conflicting viewpoints wrestle with experimental facts; (2) the "strategic," or metatheoretical, level, where the theorist withdraws to map the long campaign. The two levels appear both in the content and structure of the book. Starting on level 2 with Watson's challenge to the introspectionists, the author revisits it halfway through his survey to debate the merits of axiomatic and empirical approaches, and returns to it in the final chapter to draw up the balance sheet for S-R theory, diagnose its defects, and propose a remedy.

Meanwhile, on level 1, we are reminded of the vicissitudes of the Law of Effect, from rats that form hypotheses and learn mazes by trolley to rats that learn fear. Indeed, the central theme at this level might be called "the rise and fall of the Law of Effect." It enters the discussion of reasoning as we test Hull's notion of a goal-anticipatory response (r_g) against the performance of Maier's—and Wyckoff's—rats. Later on the focus shifts to the brain; then to behavioral dynamics and a rat's reluctance to do the same thing—or visit the same place—twice in succession.

Readers familiar with this territory should still find enough new ideas in the present treatment for positive reinforcement. A large share of these may be traced to the author's background in information theory. As a sample, here are a few items that struck the reviewer with fresh impact: (a) A lucid discussion of operational safeguards in the use of introspection. (b) Random variation and selection as a recurrent theme, not only in the evolution of species, but in instinctive behavior, in learning, and possibly, in the structure of the individual cortex. (c) An example of reinforcement without learning: the absence of a CR when the weaker component of a compound CS is tested in isolation. (d) The question of noncontinuity in discrimination learning reopened by the hypothesis that it depends on the number of competing cues.

Although Skinner's unfettered (?) approach and the current stochastic models are not neglected, Hull's system receives the lion's share of critical appraisal. Broadbent takes issue with Hull at two main points, one on each level of treatment. On level 1, after reviewing the evidence for internal mediating responses, he rejects the usual attempt to subsume them under the same rules as overt ones. To the reviewer this part of his argument, drawing heavily on Deutsch's analysis of r_g, is not too convincing, especially in the light of Ritchie's discussion also cited. But the author's basic objection cuts deeper: he holds that Hull's "open-chain" system provides a less efficient analogue of the nervous system than does Deutsch's servo-mechanical model or his own filter theory. He would concede, in his disarmingly reasonable way, that the difference may be ultimately semantic, but he would insist that it is nonetheless decisive.

On level 2 Broadbent's charge is the familiar one that Hull formalized his system too early and in too much detail. More arresting is the author's opinion of what is wrong with a prematurely formalized system. It seeks exact prediction at a stage when the need is for crucial experiments to decide between broader assumptions. One does not start a game of Twenty Questions, as the author has elsewhere pointed out, by asking if the object is one of the Crown diamonds. Here is a message from information theory that surely deserves to pass our conceptual filters.

The book was a pleasure to read. It was not intended to be scholarly; the scholarship is there, but latent. Solidly based on experiments, the argument is never lost in detail, and documentation is limited to a few suggestions for further reading. Broadbent is fair about this; he offers an appetizer, not a full meal. And appetizing it is. Written in an engaging style but without condescension, the book has a flavor enhanced by the author's flair for apt analogy. As a stimulating introduction to behavior theory it can be recommended to onlookers and participants alike.

Logical Laughter

Marie Collins Swabey

Comic Laughter: A Philosophical Essay. New Haven, Conn.: Yale University Press, 1961. Pp. v + 251. $5.00.

Reviewed by JACOB LEVINE

Marie Collins Swabey, the author, is Professor of Philosophy, Emeritus, at New York University. Her earlier books include Judgment of History *(1954) and* Logic and Nature *(1955). The reviewer, Jacob Levine, took his doctorate in physiological psychology with K. S. Lashley at Harvard but during and after the second World War moved into clinical psychology. His half dozen papers on psychological aspects of humor demonstrate a persisting theoretical concern with humor which, his friends will testify, is combined with his practice of it. He is now Chief of the Psychology Service at the VA Hospital at West Haven, Conn. and Associate Clinical Professor at nearby Yale University.*

A SCHOLAR once remarked to Samuel Johnson, "You are a philosopher, Dr. Johnson. I have tried, too, in my time to be a philosopher, but I don't know how, cheerfulness was always breaking in." Cheerfulness may interfere with philsophizing, but philosophers have not thereby been discouraged from trying to understand the meaning of comic laughter. Unlike psychologists who do not have the excuse of interference by cheerfulness, philosophers have taken the problem of humor seriously and consider it to be important.

Prof. Swabey is a philosopher who takes the comic very seriously from the conviction that "humor in its highest forms may concern itself to a degree with issues of the soul and ultimate destiny." She states forthrightly that her approach is "strictly philosophical" to the exclusion of psychological, physio-

logical, and sociological considerations. This would indeed be a remarkable feat if it could be achieved. Her basic thesis is that comic laughter is a rational activity which can be fully explained in terms of logical thought. It is an intellectual process in which the incongruous and the illogical are perceived and rejected. As a confirmed rationalist, she holds with Descartes that man is first and last a reasonable being. He laughs when he sees anything that is incongruent with reason and with the logical order of things. She is suspicious of the ability of empiricism and the natural sciences to comprehend the comic. Prof. Swabey strongly objects to evolutionist, naturalistic, empirical, mechanical, psychosomatic, and psychoanalytic views of humor. Thus, both Spencer and Darwin are severely criticized because "incongruity is interpreted psychosomatically rather than logically." Darwin is vulnerable in her eyes because for him "laughter is treated predominantly as an expression of the emotions." She feels that her objections to evolutionary thinkers run "very deep" because they are concerned with the biological origin and basis as well as the "functional utility of laughing as discoverable by genetic psychosomatic analysis": furthermore, for them, "reason is held to be derived from nonrational sources." It is understandable that she takes strongest exception to Freud, since "to Freud wit and the comic appear predominantly as enemies of reason." The notions of an unconscious or of repressed wishes are vigorously rejected since they are beyond the reach of the source of comic laughter, rational thought.

Prof. Swabey assumes a Platonic view of the comic and is concerned with what deserves to be laughed at. The comic, like beauty, religion, morality, and truth are positive values, and are all forms of the basic coherence and harmony of the universe. She therefore finds the modern artist who turns away from classical harmony and objective representation as either sick or a fraud. It is understandable that from this idealistic viewpoint obscene humor is really not humorous, and her objection to "amoral jesting is that it is finally self-stultifying." For her, "what is really important . . . is that in the laughter of the comic insight we achieve a logical moment of truth." For myself, I find it hard to think of a good belly laugh as an "intellectual victory and the rejection of illogicality."

A New Look at Murder

Walter Bromberg

The Mold of Murder: A Psychiatric Study of Homicide. New York: Grune & Stratton, 1961. Pp. v + 230. $4.75.

Reviewed by RALPH S. BANAY

The author, Walter Bromberg, is a psychiatrist who has had extensive experience, both as practitioner and teacher, with matters of criminology. His earlier books include Crime and the Mind *(1948),* Man above Humanity *(1954), and, with J. K. Winkler,* Mind Explorers *(1939). Ralph S. Banay, the reviewer, also a psychiatrist, is Director of the Youth Institute and of the Civic Center Clinic in Brooklyn and serves as Editor of the* Journal of Corrective Psychiatry and Social Therapy. *Earlier he was a research psychiatrist at Columbia University, where his work centered on social deviation patterns.*

FOR DR. BROMBERG, the phenomenology of murder is no armchair exercise but a field in which his professional experience has afforded exceptional opportunities for first-hand observation and informed judgment. As a past director of the psychiatric clinic of the Court of General Sessions in New York, as a correctional therapist in the Navy during the war and as a training consultant to the California Department of Mental Hygiene, he has been in active official contact with celebrated and diverse cases. His book, therefore, profits from a privileged, authoritative viewpoint in presenting a wide range of analysis and commentary based on details of the outstanding murders of the last thirty years, together with others that have involved moot medical problems.

Two of Dr. Bromberg's focal observations are especially admonitory: that murder is one of the nation's most expensive luxuries, and that it is the one social plague that has eluded prevention. He invites us to ponder the paradox that although every killing is elaborately analyzed, accumulating a wealth of knowledge on the social and pathological dynamics of imposed death, and although correctional efforts are constantly being multiplied to deter the violent impulse, society has been unable to check the rising incidence of capital crime. The basic problem is to find a means of preventing the taking of life, and that quest so far has been largely futile.

Dr. Bromberg realistically considers that the high homicide rate—4 per 100,000 of population in 1930, 8.5 in 1954 and 7.3 in 1958—is not substantially reducible by the means so far mobilized against it. Ruminating on the familiar fact that everyone is obsessed to some degree with the idea of violent death, he relates this to the closeness of the aggressive drive to our innermost psychological core. The criminal acts out those impulses and fantasies that the law-abiding citizen represses and abhors —"society loves its crime but hates its criminals." Thus the average man finds in the panorama of crime a defense against his own repressed inclinations. This reciprocal reaction between the unconscious preoccupations of society with violence and the antisocial impulses of the actual criminal, Dr. Bromberg suggests, results in a kind of homeostasis, "for truly society, for its own equilibrium, needs the criminals it spawns."

"The difference between the 'dreamer' and the doer of violent crime," says Dr. Bromberg, "resides in the uniqueness of each individual—in his ego strength, predominant social images, the vigor of his repressive mechanism, balance of the various aspects of his ego and so on. But in the concordance of the substratum of fantasy and act . . . can be discerned a buried Program for Murder in all humanity."

Dr. Bromberg proposes that the implements of dynamic psychology may

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yet provide the answer that education, exhortation and precept, as well as the impact of psychiatry, have failed to provide in reducing the prevalence of violent crime. He suggests that the elements of psychodrama could be employed in an educational campaign comparable with those that have reduced such other social plagues as cancer, heart disease and polio. Such a project, he submits, would call for a reorientation of the presentation of crime so that the individual's and society's part in it—rather than the elements now played up in fiction, drama and the news—would be brought home to everyone. He believes that possibly a decade of this type of presentation of "the psychological cosmos of crime" would make it less easy for the common man to isolate himself from wrongdoing and to dismiss it from his mind.

Practitioners and laymen interested in the relationship between mental disorder or defect and the commission of crime will find much to interest and instruct them in Dr. Bromberg's book. His observations on the psychopath, on epilepsy and other malfunctions and on the irresponsibility of both psychotics and the subnormal are especially timely and pertinent. "The type of mental disease found among murderers represents a panorama of psychiatric diagnoses," he concludes. "The more one studies 'senseless murders,' the more it becomes evident that the extreme concentration of hostility which is exploded in such a murder stems from a schizophrenic process . . . It is an academic discussion whether such murderers are preventing an outbreak of schizophrenia or are already schizophrenic . . . It seems a twist of psychological fate whether in these sick individuals suicide or murder ensues. In fact, it has become a psychoanalytical axiom that 'nobody kills himself who has not intended to kill somebody else.' From the criminological point of view the reverse is true, especially among psychotic individuals."

It is among rascals that one finds the most fools.

—Jules Verne

A Taxonomist in Africa?

Leonard W. Doob

Communication in Africa: A Search for Boundaries. New Haven, Conn.: Yale University Press, 1961. Pp. vii + 406. $7.50.

Reviewed by M. Brewster Smith

Leonard W. Doob

The author, Leonard W. Doob, studied at Dartmouth College, Duke University and Harvard University, receiving a degree from each, with the PhD coming from the latter institution. After teaching for a short period at Dartmouth, he moved to Yale in 1934 and has been there ever since. Among his earlier books are Public Opinion and Propaganda *(1948),* Social Psychology: An Analysis of Human Behavior *(1952) and* Becoming More Civilized *(1960). M. Brewster Smith, the reviewer, besides being a regular reviewer for* CP *has other accomplishments to his credit. His past includes the editorship of the* Journal of Abnormal and Social Psychology, *the presidency of SPSSI and the vice-presidency of the Joint Commission on Mental Illness and Health. It also includes a co-authorship, with Samuel Stouffer, of* The American Soldier *(1940). His present is occupied with the demands of being a professor at the University of California in Berkeley and of serving as the Associate Director of the Institute of Human Development. His future will involve, among other things, a report of a follow up of Peace Corps activities in Ghana.*

This odd, interesting, and only partially satisfying book is in at least one respect a very personal one, representing as it does the juncture of dominant themes in Leonard Doob's work of almost thirty years as a social psychologist. *Communication:* the book carries forward the ever-expanding progression from *Propaganda* (1935) through *Public opinion and propaganda* (1948) to a point at which communication becomes a perspective from which to view the entire gamut of human behavior and material culture. *Africa:* Doob has been a leader in the pitifully small band of psychologists that saw "early on" the challenge and opportunity of cross-cultural research as the harsh light of the modern world burst upon the Dark Continent (see his recent *Becoming more civilized). Taxonomy:* from the beginning, Doob has been the taxonomist *par excellence,* formulating in his first book a set of classificatory principles of "propaganda," and now seeking to illustrate with African materials an inclusive array of potentially strategic factors in the communication process.

The trouble is, this confluence of themes is more intelligible in terms of personal biography than of a unified conception. Superimposed as they are, the themes remain orthogonal in this book that is at once an implicit argument for an expanded view of communication, a trial run of a descriptive system for classifying its variables, and a miscellaneous catalogue of lore and exampⁱe—a sort of Fraser *cum* media guide—concerning Africa south of the Sahara.

There is, to be sure, a unifying rationale for the book. The student of communication, says Doob, needs an inclusive and ordered check-list of variables that may become strategic in influencing the process and outcome of communication: not that he has to take all of them into account in any single study, but if he possesses an adequate array of conceptual categories he will be less likely to neglect important influences unwittingly. Sub-Saharan Africa becomes the testing ground for this scheme which is a direct outgrowth of Doob's earlier writings on communication and propaganda. Examples from a fresh terrain not only sharpen our attention to aspects of communication that we may take for granted in our own accustomed cultural process; they also help to assure the pancultural applicability of the categories.

Doob organizes each of the chapters of the book around one of twelve classes of variables that, in his model of the communication process, fall in a closed circle: the communicator, the goal, basic media, extending media, the site, restrictions, the communication, mood, perception, reactions, changes, feedback. Each of these rubrics is extensively subdivided. But apart from a brief and informal prologue, the scheme itself is neither discussed nor defended; Doob does not share with us the decision processes that led him to choose this particular formulation rather than another. It seems fair to say that while the scheme as a whole is offered with due tentativeness and modesty, each element is presented dogmatically.

It is therefore hard to evaluate the success or utility of the taxonomic contribution. The reviewer could find little reason to quarrel with it, other than the inevitable reaction that in some respects it seemed pedantically detailed, in others gross or superficial.

But taxonomy, though it is the avowed rationale for the book, is only the skeleton; the meat rather loosely hung on the taxonomic bones is the illustrative material culled from the anthropological literature, from accounts of missionary activity (introduced somewhat in the spirit of recurrent comic relief), and from the slim documentation available on modern mass communications in Africa—documentation that as Doob points out is far from adequate to give dependable snapshots of a situation that is also in rapid flux. By providing a bibliography of this literature and a critical guide to it, Doob makes a valuable contribution to those whose work makes them need an introduction to it. A small amount of fresh data from his own African investigations is also included. From the perspective of Doob's avowed purpose of illustrating his classification scheme, it is legitimate enough to lift examples out of context as he does.

This melange, presented with Doob's characteristic light touch, is inherently interesting, with the fascination for the layman of any collection of strange customs. From it, the reader will learn much about characteristic African institutions and environments, and something of the heterogeneity that makes it so dangerous to generalize about the continent. While some of the discussion makes no claim to be other than superficial, in general one has the feeling that Doob is a sophisticated as well as a genial guide.

But the *potpourri* that does well enough to *illustrate* is less than satisfactory for a substantive view of African communication, in which readers will be more likely to be interested than in taxonomic matters. There is, for example, no way of telling the extent to which the anthropological accounts of tribal life, heavily emphasized, continue to be relevant. There is no treatment of African cities as a context for communication, and, for that matter, no frontal consideration of continuity and change in African communication systems. Unfair as it may seem to take the author to task for failing to accomplish something he never intended, the reader still needs to be warned that the book is not an analysis and summary of what is known about contemporary communication in Africa.

The "search for boundaries" to which the title of the book alludes refers to conceptual boundaries, not geographical ones. What considerations, what factors and types of factors, have to be included in a full account of the process of communication, on the grounds that they have in some instances been shown to make a difference? This is Doob's central problem. The path he has taken leaves a dissatisfaction with his metaphor: no boundaries emerge from his African safari. While Doob does not employ the currently stylish language of "information theory," it is clear that he shares implicitly the view of those who use it: the transmission of information is an aspect of behavior, culture, and many natural processes; it is not a bounded region.

If Doob was really not seeking boundaries *or* finding them, he was scanning savannah and jungle for variables and a way of ordering them. Few will cavil at the variety of variables he encountered. As for his way of ordering them, each will have his own preferences. The proof of the pudding is in their usefulness in a context of research and theory of a kind that Doob has not attempted in this book.

Intelligence on Abilities

Philip E. Vernon

Intelligence and Attainment Tests. New York: Philosophical Library, 1961. Pp. 207. $7.50.

Reviewed by JULIAN C. STANLEY

The author, Philip E. Vernon, is a British psychologist well known to many American colleagues since his collaboration with Gordon Allport on The Psychology of Expressive Movements *(1933) and on the Allport-Vernon (now the Allport-Vernon-Lindzey)* Study of Values. *His more recent books include* The Structure of Human Abilities *(1950) and* The Measurement of Abilities *(2nd edition, 1956). Julian C. Stanley, the reviewer, enacts multiple roles at the University of Wisconsin; among other things, he professes Educational Psychology, directs the Laboratory of Experimental Design and chairs the department in which he professes. There is also multiplicity as well as versatility in both his research and his off-campus professional activities. His research has tended to concern itself primarily—but not exclusively—with problems of measurement. His professional activities have carried him into sometimes technical, sometimes honorific relations with such organizations as APA, the College Entrance Examination Board, the US Office of Education and the National Council on Measurement in Education.*

IN JUST 185 small pages of text this British educational psychologist, well known to many of us because of the 1931 Allport-Vernon Study of Values inventory and his several books, discusses mental ability so lucidly and insightfully that intelligent laymen, students, and specialists will all find much of interest. Vernon communicates well without oversimplification. His style is straightforward and declarative, but typically he tries to include several points of view. Notably, he buttresses his arguments by citing nearly two hundred books and articles, many of them British studies not widely known in the United States.

Vernon deftly weaves history, statistical analysis, experimentation, and theory together so that the result is less obviously a beginning textbook than are most of the ones published in this country, but at the same time it is not a highly specialized treatise or manual such as the 1937 and 1960 Stanford-Binet volumes. Perhaps the closest approach we have is Florence Goodenough's much longer, more academic *Mental Testing* (1949)— which, *mirabile dictu,* Vernon does not even list in his bibliography!

He stresses 'intelligence' testing throughout, despite the title, except for the lonesome ten pages that constitute Chapter 6, "Educational Attainment

Tests," in which some popular topics, largely irrelevant to his main theme, are dispensed with quickly.

On pages 39 and 41 Vernon states the following neoSpearmanian view, which serves as a unifying thread for what might easily have been a loose collection of diverse remarks about many topics:

. . . intelligence corresponds to the general level of complexity and flexibility of a person's schemata, which have been built up cumulatively in the course of his lifetime. It would follow that no sharp distinction should be drawn between intelligence and attainments; nor should we think of the former as one of the main causal factors in determining the latter. Both are dependent on, or limited by, genetic factors . . . and it is probable that various specialized attainments involve particular genes in addition— e.g. those underlying musical or numerical talents. But there is no essential difference between the acquisition of, say, reading skills and the acquisition of reasoning or other capacities which would be conventionally regarded as part of intelligence. Both involve the development of schemata through exercise with appropriate materials, and their shaping or correcting by environmental pressures. . . . *g* can be regarded as the highest common factor among, or as the overall efficiency of, schemata.

In the excellent chapter on heredity and environment, he judiciously explores the nature-nurture balance hinted at above, emphasizing "the importance of upbringing, social differences and education." Vernon is by no means an extreme environmentalist, however. His statement on pages 175-176, citing Shuey's 1958 book favorably, may discomfit some SPSSIites: "From such results, it seems quite probable that genetic differences between racial and ethnic groups do exist—at least in some aspects of intellectual ability—though we have no satisfactory way of proving it."

All in all, nearly every thoughtful adult would find something worth-while in this little volume. It could do much to counteract the superficial, stereotyped criticisms of 'IQ tests' rampant in the popular press of his country and ours. Present and prospective teachers particularly need Vernon's balanced evaluations.

The Art and Science of Predicting

J. T. Barendregt

Research in Psychodiagnostics. The Hague: Mouton and Co., 1961. Pp. v + 221.

Reviewed by Eli A. Rubinstein

J. T. Barendregt, the author, took his PhD degree in 1954 at the University of Amsterdam, where he worked on the problem of psychosomatic specificity. He continued his research and publication in this area while serving as psychologist in the Department of Psychiatry at Amsterdam's Wilhelmina Hospital and as lecturer at the University. The reviewer, Eli A. Rubinstein is a clinical psychologist with a PhD from the Catholic University of America, and now is Assistant Chief of the Training Branch of NIMH. Before becoming embroiled with administrative processes he spent seven years in the Veterans Administration psychology program, meeting during that period the dual challenges of service and of research, the former transpiring in a Mental Health Clinic and the latter dealing with outpatient treatment procedures. He co-edited Clinical Practices in Psychology *(1954) and, with M. B. Parloff,* Research in Psychotherapy *(1959)* (CP, *Mar. 1960, 5, 74).*

If clinical psychology is still more of an art than a science, then this collection of research studies can be considered a charming illustration of the 'modern primitive' school. The volume consists of thirteen individual reports on a variety of investigations, from an examination of the accuracy of clinical prediction to an effort to predict the degree of reaction to LSD-25. In between are such varied portraitures as: "The Rorschach Test as a Means of Testing the Hypothesis of Psychosomatic Specificity;" "An Experimental Study of the Halo Effect;" "A Contribution to the Nomological Net Surrounding 'Neuroticism';" and "A Psychological Study of the Effect of Psychoanalysis and Psychotherapy."

The author, who did these studies in collaboration with others, makes some apology in the preface about conditions under which the research was accomplished. The work was carried out in the Netherlands and "the choice of subject often depended more on an opportunity for investigation than on the programme I had in mind. Library and computational facilities were rather limited." The evidence of these limitations is most apparent in the fact that very few of the references are later than vintage 1956, although for many of these studies, highly relevant research has been published more recently. Furthermore, lack of programmatic continuity only emphasizes the limitations, since these studies themselves don't build one on the other.

The author's major objective is to demonstrate that clinical prediction does not work, while objective methods using "nomological networks" show more positive results. However, the latter group of experiments are not any more significantly positive, than the former are significantly negative.

Concerning the clinical studies, there are some serious deficiencies in presentation as well as in experimental design. In the first place, it is all chiaroscuro—with an emphasis on the black tones. The clinician is depicted as a rather inferior predictor. The Rorschach is not an efficient instrument in discriminating between psychoneurotics, asthmatics, and psychosomatic patients. There is no relationship between accuracy of and confidence in clinical prediction.

Now, none of these conclusions in the context of the major objective are new or startling. And that perhaps is a crucial weakness of this entire effort. How many times, since Meehl weighed the clinician on the statistical scale and found him wanting, has someone pointed out that one good regression equation in the hand is worth two bushy-browed clinicians? (None of these findings, incidentally, including that of Dr. Barendregt's, have improved on

the original home-cooked Meehl.) How many times has the Rorschach failed to discriminate between ill-defined diagnostic groupings? And how many times has it been shown that clinical accuracy is not a function of clinical confidence, or vice-versa?

In the light of this negative comment it might be asked what this reviewer found 'charming' in this series of studies? Perhaps it is the earnestness with which the author tried to depict the true image of psychodiagnosis. And, perhaps, despite the broad brush strokes, plus the errors in perspective, plus the over-emphasis of the obvious, these little studies are rather accurate representations of this part of today's world of clinical psychology.

If the naiveté of the 'modern primitive' school does, indeed, capture the essence of reality, then the judgment that these studies are charming needs itself to be looked at somewhat critically. Could the reviewer be defending himself against some underlying feeling of discomfort that even a more sophisticated set of experiments would have ended up with much the same results? If so, what does this suggest about our ability to substantiate the effectiveness of clinical predictions? Can we here in the United States, with all of the clinical research now underway, provide more positive evidence for psychodiagnosis than is portrayed in this book? The readers with competence in clinical psychology are invited to answer that question for themselves after having read *Research in Psychodiagnostics.*

Idiographic Humanistic Personality

Harold Grier McCurdy

The Personal World: An Introduction to the Study of Personality. New York: Harcourt, Brace & World, 1961. Pp. v + 591. $7.50.

Reviewed by J. G. Schimek

The author, Harold McCurdy, has long lived, worked and written in the State of North Carolina. He took his degree at Duke University and is presently a professor of psychology at the University of North Carolina. His earlier books include The Personality of Shakespeare *(1953) and a volume of poetry. He is now a six-time reviewer for* CP, *with the most recent of these appearing in* CP *at about the same time as his present book* (CP, *April, 1962,* **7**, *161.) The reviewer, J. G. Schimek, did his undergraduate work in Switzerland and France where he began a persisting involvement with literature, history and philosophy. He received his PhD in psychology in 1954 from the University of California at Berkeley. He has served as a clinical psychologist at the Austin Riggs Center and is now Chief Psychologist at the Albert Einstein Medical Center in Philadelphia. His current research deals with personality correlates of subliminal perception and his current writing, being done in collaboration with Lester Luborsky, deals with the psychoanalytic theory of personality change. The latter project is due to become a chapter in a forthcoming volume on personality change.*

This is an unusual, original and very probably a controversial book, hard to fit within the confines of even such a broad and undelineated field as personality psychology. Generally speaking, it follows the idiographic, humanistic orientation as opposed to the behavioristic, trait-measuring and factorial approach. More specifically, the author seems to have been greatly influenced by McDougall, William Stern and certain aspects of European phenomenology. He wants to limit himself to a description of the immediate data of experience in their life-like complexity without narrowing them in advance to the concepts of a specific theory, and without reducing them to fit the requirements of quantitative experimental methodology. His primary aim is "a sort of understanding or appreciation, nearly aesthetic, of the total picture," in terms of the various levels on which the individual experiences his personal world and relates to it.

The author's data range from experimental and quantitative studies to clinical observations and descriptions, with the latter taken both from literature and personal experience. He relies on many unjustly forgotten or little known sources, particularly European ones (e.g., modern French characterology). His practical and didactic goal is to present the student with vivid, challenging and often unfamiliar data which will raise fundamental questions and encourage him "to play with ideas." In fulfilling these aims, the book succeeds very well; it is well written and consistently interesting. It should make an excellent and necessary supplemental reading for a standard course in personality, and will serve well any psychologist wanting to have his "personal world" somewhat broadened and challenged.

In the book's first section where McCurdy deals with the biological basis of individual development, its emphasis is on ways in which heredity and maturational sequences limit and channel the influence of contingent environmental factors and, further, there is a view of instinctual drives as broad structuring tendencies of our interaction with external objects (on the cognitive as well as the motor and affective levels). Throughout the book we find vivid accounts of the compelling, peremptory "demon-like" character of many of our experiences, but unfortunately not much is said about the way these factors might tie in with drive forces and drive representations. We are left with the problem —crucial to much of contemporary psychoanalytic and learning theories alike —of reconciling drives as quantities of

Introduction to the Statistical Method

Foundations and Use in the Behavioral Sciences

by KENNETH R. HAMMOND, *University of Colorado*

and JAMES E. HOUSEHOLDER, *Humboldt State College*

Enlivened by 31 numbered boxes that feed the student gems of historical interest or dramatic pronouncements about statistics, this text provides a pleasant introduction to the subject. For reasons easy to appreciate, I like the organization. It begins with its feet on the ground in the theory of measurement and goes all the way to the theory of decision.—Stanley S. Stevens, *Harvard University*

1962; 431 pages; $7.00 *text*

Psychology in the Making

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edited by LEO POSTMAN,

University of California, Berkeley

This unique new text fills a gap which has long existed in undergraduate courses—the need for a strong introduction to the nature of psychological research. Throughout the book emphasis is placed upon *how* research is conducted rather than on the result of research. Each chapter is written by a well-known psychologist, and great events and researchers of the past are brought to life by frequent, liberal quotations from original sources.

Fall 1962; about 900 pages; $9.00 *text*

Elements of Psychology

by DAVID KRECH *and* RICHARD S. CRUTCHFIELD

both of the University of California, Berkeley

This text is a valuable contribution to the literature of introductory texts. Its coverage of . . . perception is excellent.—James Appel, *Yale University*

758 pages; $7.00 *text*

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energy with drives as structures or patterns of object relationships.

The second section centers around a description of the variety of states of consciousness, self-awareness and the experience of "I," as illustrated by data from dreams, hypnosis, mystical experiences, etc. The author states that the distinction between the "objectivity" of public behavior and the "subjectivity" of private experience is by no means an absolute one, but is only a question of the degree and ways in which different levels of experience can be shared and communicated. This implies that "objective" behavior too is only known as part of someone's consciousness and really refers to those aspects of experience (primarily gross motor actions) which have been singled out because they can most easily be consensually validated and, hopefully, predicted and controlled. One wishes the author had not been so shy of theory and had tried to unify his descriptive data into some explicit formulation of the development of consciousness and its relationship to personality dynamics. On a more specific matter, one can regret the rather narrow and one-sided presentation of Freud's theory of dreams as "a jumble of symbols" serving to disguise instinctual wishes. He could easily have adopted the broader view of dream work as a primitive, regressive form of thinking under the conditions of sleep. Also it would have been welcome to have some reference to contemporary psychoanalytic ego psychology (for instance, the work of Rapaport, Hartman and Erikson) and its attempts to deal with the problems of self, identity and consciousness.

In discussing methods of describing personality, the author's general approach is that tests, although they have some practical usefulness, should be subordinate to an intuitive understanding of the individual self; but he does not suggest how this might be done. He describes the intuitive attitude as being attuned to any clues from the other person (somewhat like the "free-floating" attention which Freud recommended to psychoanalysts); we should not restrict ourselves in advance, he holds, to those areas of a person which fit our limited interest in controlling or manipulating him.

The final section deals with phenomena on the "borderline of science," such as telepathy, mediums, mystics, etc. The author feels that these are important areas of human experience which should not be ignored or dismissed with the label of "subjective" or "pathological" simply because we do not yet have reliable observations and adequate concepts to deal with them.

As is apparent by now, the reviewer sympathizes with the author's desire to stay close to the direct description of lived experiences and to avoid the sacrificing of meaning and content for the sake of methodological purity. However, such an approach raises some basic problems not clearly faced in the book. First, in contrast to a myth often shared by behaviorism and phenomonology alike, there is no such thing as "a raw fact;" all data presuppose a selective orientation and all rest on initial assumptions which are often implicit but should be stated explicitly in the interest of communication and objectivity. Secondly, it is not clear what the author means by "an intuitive science based on non-behavioral methods;" there is not enough of an answer in his personal belief that understanding requires a loving acceptance of the other person as part of our loving acceptance of the world or God in all their aspects. Is it not feasible and necessary to compare and cross-check our initial intuitive knowledge of mental processes with that of other observers, and with information obtained by other methods? We need concepts and techniques (tests, rating scales, questionnaries, etc.) to help us put our intuitive understanding—or at least some parts of it—in a form that can be communicated and validated. Even if one agrees that the search for elementary, discrete "traits" or S-R units is rather sterile, one still needs some dimensions, however preliminary and tentative, along which to describe, classify and study personality functioning. Such a basic dimension might be found in the concept of a hierarchy of levels of consciousness or ego states ranging from the "objectivity" of pragmatic action-oriented perceptions to the "subjectivity" of memory, phantasy and dreams. The general quality of all cognitive experiences is that they refer to something beyond themselves; the immediate content of our awareness is the object "out there," not the changes in our brain, sensory receptors or viscera. For each level of consciousness we can find consistent formal structural characteristics depending on the way in which the object is experienced (i.e., how vivid, compelling or stable, how much it is felt as part of our self or independent of us, and how much it is communicable and in agreement with other people's experiences). The fluctuation between these levels of experience and their specific organization within the individual can be meaningfully related to general developmental and dynamic principles (c.f., Piaget's schemata and Rapaport's cognitive structures). McCurdy seems to imply an approach of this kind, but one is never quite sure.

There has been in some quarters a breakdown of belief in the possibility of communication between people. We are told it is impossible to reach a massive audience, that the only way we can reach great numbers of people is to stoop or pander to them. That is not in the least true. Think of Dickens, Shakespeare, Sophocles, or Molière. They worked with a large public, did what they wanted to do, and did it so superbly well that we still see their plays and read their novels. Tolstoy was a successful novelist until he became the spokesman of a cult and then he became less effective as an artist.

—Walter Kerr

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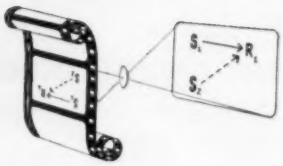
INSTRUCTIONAL MEDIA

Edited by A. A. Lumsdaine



An Olla-Podrida of Autoinstruction

Joseph W. Rigney and Edward B. Fry

Current Teaching-Machine Programs and Programming Techniques. Washington, D. C.: National Education Association, Department of Audio-visual Instruction, 1961. (Supplement 3, *Audio Visual Communication Review;* Vol. 9, No. 3.) Pp. 119. $2.00.

Reviewed by DAVID ANGELL

Rigney, the senior author of this volume, is Adjunct Associate Professor of Psychology and principal investigator of the Electronics Personnel Research Group at the University of Southern California, where he received his doctorate in 1953 after taking a master's from the University of New Mexico. Like the previous AVCR Supplement which Rigney co-authored with E. B. Fry and Glenn L. Bryan (see CP, *June 1960,* **3,** *206-207), this one was originally produced as a report for the Personnel and Training Branch of the Office of Naval Research. Fry, the junior author, also is a USC PhD, his dissertation being one of the first devoted to variables in self-instructional materials. During the past year he has been at Makerere College, Uganda, as a Fulbright lecturer while on leave from his post as Associate Professor and Director of the Reading Clinic at Loyola University, Los Angeles. Angell, the reviewer, is Research Scientist for the Instructional Methods Program at the Palo Alto office of the American Institute for Research. A PhD in experimental psychology from the University of Connecticut, he joined the AIR staff in 1959 after four years at the Human Resources Research Office of George Washington University. At AIR he has helped develop autoinstructional programs for SAGE-system operators and, for the past three years, has been engaged in experimentation on cueing techniques sponsored by the Air Force Office of Scientific Research.*

IN the preface to this AVCR supplement, the authors state that the report "is a survey of teaching-machine programs and programming techniques as they existed at the end of 1960." That it is more a survey of the former than of the latter is evidenced by the fact that nearly 75% of the article's pages are devoted to excerpts from 81 autoinstructional programs. These 84 pages constitute Part II of the two-part report. Part I (27 pages) comprises the programing-techniques aspect of the survey. The text of Part I and the program excerpts of Part II are conceptually as well as spatially distinct. Except that each program in Part II is classified by the authors as either a constructed-response program, a multiple-choice program, or a scrambled-book program, the distinction between these modes of responding and program formats having been drawn in the text, there is no integration of Parts I and II, and there are no cross-references.

The reviewer sees this lack of integration as an important shortcoming of the report, for this reason: various programing concepts and program-writing techniques—vanishing, branching, conversational chaining—are discussed in Part I, and examples of their use appear in the program excerpts of Part II, but it is left to the reader to locate and identify these examples. This reader feels that program characteristics would have been more thoroughly illuminated, and programing techniques more adequately described if, as the characteristics and techniques were discussed in the text, relevant examples from actual programs were brought to the attention of the reader.

To be sure, the authors *do* illustrate some of the programing techniques they discuss. Better illustrations, however, could have been drawn from the large population of programs to which the authors had access. The use of vanishing (the progressive frame-to-frame reduction of stimulus support), for example, is unclearly and imperfectly illustrated in the text. But the Squaring program by James L. Evans on pp. 73-74 provides some fine concrete examples of the technique in practice. Another illustration in the text is of a matching type of multiple-choice item. This example can quite confuse the reader who pays attention to the program-writing advice offered by the authors on the succeeding page. They warn, in the section headed *Practical Considerations for Programmers,* that the beginning programer "is quite likely to write the first version of his program with steps that are inappropriate, too difficult, and too few for the material. He may violate the rule that steps must be so easy that the students seldom, if ever, make mistakes in answering them *(quite different from item writing for a test)*" (italics added). But the example of a multiple-choice item from an arithmetic program, one page earlier, obviously consists entirely of test items; there is not an instructional frame in the lot. When this program segment appears again in Part II, a parenthetical explanation makes clear that the item is *not* from a self-contained autoinstructional program. This fact, however, is nowhere mentioned in the text, and the example, taken together with the statement quoted above, might lead the reader to infer a philosophy of "Do as we say, not as we exemplify."

The *Practical Considerations* section contains both some how-to-do-it (and

how-not-to-do-it) suggestions by the authors and some programing recommendations (originally published elsewhere) by Holland, by Evans, Glaser, and Homme (the RULEG system), and by Crowder (intrinsic programing). In addition, the section presents some data (gathered, presumably, via questionnaire) relating to the amount of time which programers find is required to produce both rough and finished frames. There are long quotations from several respondents. The gist of the questions asked is evident from the content of the answers, which is fortunate, since the questionnaire is not reproduced. Then there is a table which gives, for each of twelve respondents, the total number of steps (items, frames) he had written, and the number of both rough-draft and polished frames produced in a man-day. No summary statistics are given, but here are several, quickly calculated, to show the central tendency of the distribution of responses to the total-steps-written question:

$$N = 11$$
$$\text{Median} = 4{,}000$$
$$\text{Mean} = 97{,}755$$

The skew is produced by one extravagantly prolific respondent who reports having written 1,000,000 frames, or 977,000 more than the next most productive frame writer. That total is, in fact, about 850,000 more frames than there are in the 81 complete programs from which excerpts are printed in the report. One wonders how the question was worded to have elicited this idiosyncratic response, and why the datum was reported without comment or explanation.

The final section of Part I contains a classification, in outline form, of major programing variables. The classificatory scheme is conceived as a basis for (1) comparing programs, and (2) suggesting areas where research is needed. It should serve both purposes, and it is indeed an ambitiously thorough outline. But completeness can involve some farfetchedness, and this compulsively total outline has not escaped that pitfall. The reviewer questions, for instance, that "bioelectrical responses (e.g., GSR, EEG)" will ever be considered as a response mode by autoinstructional programers ("If alternative A is correct, generate an alpha wave," maybe?), and considers it improbable that it will turn up as a variable in programing research. There are other instances in this exhaustive catalog which seem more pretentious than heuristic.

In Part II, frames from 62 constructed-response and 19 multiple-choice programs, in 21 subject-matter areas, are given. The median number of frames per program sample appears to be about 8, which in many cases is a tiny fraction of the total program. This brevity would not be a problem, since the frames are but illustrative, if some indication were given of what, specifically, they were illustrative of. They are, however, reproduced without comment. Selection would seem to have been preferable to exhaustiveness, and commentary to standoffishness. It is evident that the collection of program samples is the *raison d'être* of the report, and despite the shortcomings of the section containing this collection, it is the report's *grâce justifiante* as well. Incidentally, the program samples are arranged alphabetically by general subject matter and then by author's name. One may ponder the significance of the collection's being anchored fore (Auto-Instruction) and aft (Statistics) by Sidney L. Pressey. He *did* have the first word, didn't he? And the last is yet to come.

The report contains a 35-item bibliography (34, if you don't count Guilford's *Factorial angles to psychology*, an article whose relevance to autoinstruction is nonexistent and whose inclusion in the bibliography must have been inadvertent) and a useful index to the subject matter of the program samples, their authors and institutional affiliations.

The reviewer feels that there are better articles than this in the genre, for example, those by Silverman [*Automated teaching: A review of theory and research;* Technical Report, U.S. Naval Training Devices Center; June 1960] and Stolurow [*Teaching by machine;* U.S. Department of Health, Education, and Welfare, Cooperative Research Monograph, 1961; see *CP*, June 1962, 7, 243]. But the latter don't have excerpts from 81 programs.

Thorndike's Dream Realized?

James D. Finn and Donald G. Perrin

Teaching Machines and Programed Learning, 1962: A Survey of the Industry. Occasional Paper No. 3, N.E.A. Technological Development Project. Washington, D. C.: National Education Association, 1961. Pp. 85. $1.50 (multilith; spiral bound).

Reviewed by JOHN E. COULSON

Finn, the senior author, is Professor of Education and Chairman of the Audio-Visual Department at the University of Southern California. At present he is on academic leave serving as principal investigator for the National Education Association's Technological Development Project, which is collecting and collating information on the history and current trends in instructional technology. Dr. Finn, author of many articles in the audio-visual and educational-technology fields, recently served as president of the NEA Department of Audiovisual Instruction. Perrin, his co-author, is currently Research Associate for the Technological Development Project while working toward his doctorate in communications at USC. He has produced several educational films, including some on science for the Australian government. Coulson, the reviewer, is a Columbia PhD in psychology who is now Associate Head of Educational Research and Development for the System Development Corporation in Santa Monica, California. He helped initiate the SDC Automated Teaching Project in 1958 and, in collaboration with H. F. Silberman, published one of the first of the new wave of experimental studies on teaching-machine program variables. More recently, he has helped design the "CLASS" computer-based teaching-system laboratory at SDC and edited the ONR-SDC symposium volume, Programmed Learning and Computer-Based Instruction *(Wiley, 1962).*

A GROWING count of theses, dissertations, journal articles and reference books attests to the popularity of programed instruction as a subject for experimental research. More recently, interest in programed instruction has spread to the ranks of practicing teachers and to the general public, largely as the result of popular articles proclaiming a coming revolution in education.

Unfortunately, neither the popular accounts nor the research literature provides a clear, realistic picture of the availability of commercially produced programs and teaching machines for here-and-now application in the classroom. In the present occasional paper Dr. Finn and his associate, Mr. Donald Perrin, have taken a useful step toward filling this gap. The authors are well qualified for this task. Dr. Finn, in his capacity of Principal Investigator for the Technological Development Project of the National Education Association, has visited many universities, research organizations and manufacturers, collating information about trends in programed instruction and other forms of instructional technology. Mr. Perrin, research associate to Dr. Finn, has been active in the production of educational films.

The goal of the present report, as stated by the authors, is to provide answers to research workers, curriculum supervisors, school administrators and industry itself, on (1) what is going on in the teaching machines—programed learning industry, and (2) what is to be expected in the near future.

The major portion of this paper is a directory or catalog of teaching devices and instructional programs, and a listing of producers of these materials. An interesting approach has been used in stating the criteria for inclusion of instructional devices as teaching machines. Seven "basic functions" and eight "additional functions" of teaching machines have been listed. To qualify for inclusion a device must provide at least five of the following "basic functions":

1. "Used for individual instruction."
2. "Contains and presents program content in steps."
3. "Provides a means whereby the student may respond to the program."
4. "Provides the student with immediate information of some kind concerning his response that can act as a psychological reinforcer."
5. "Presents the frames of the program individually."
6. "Presents the program in a predetermined sequence."
7. "Is cheatproof."

"Additional functions" listed include such characteristics as automatic advance and automatic recording of results. Eighty devices are listed in the directory as meeting the requirements of the survey of teaching machines, and almost twenty others are included as "simulated teaching machines" (e.g., programed books and separate response devices).

While the reader may argue with certain inclusions or deletions in the list of "basic functions," this general approach to the definition of teaching machines is commendable. It ensures some common basis for use of the term, without restricting the term to any particular "brand" of programed instruction.

The directory includes only machines that are commercially available. It is a rather comprehensive listing, though its accuracy can be only temporary in such a rapidly developing field. As the authors point out, teaching machine producers and program publishers appear and vanish with startling speed during this early developmental phase of the industry. A picture and brief description is provided for each machine. More detailed information must be obtained directly from the manufacturers.

THE directory of instructional programs contains two lists totaling over 600 programs. One list includes programs commercially available at the end of 1961; the other shows programs "under development" and presumably to be ready in 1962. Readers should take this second list with a large grain of salt. Program producers are notoriously optimistic about the scheduling of new programs, and some programs "under development" may never appear on the market. A further criterion stated for inclusion of programs in the directory is that they should be applicable to school or college curricula. This statement is difficult to reconcile with the paper's listing of programs on "Fundamentals of Chess" and "The Elements of Bridge."

It is unfortunate that so little information is provided in the directory about the listed programs. In many cases the directory tells only the producing company and the program title. In other cases the listing also includes the number of instructional frames and the academic level for which the program is intended, but the reader will not be able to discover what method of programing has been used (i.e., constructed response, multiple choice, branching, etc.), or what associated teacher's manuals or tests are available. Nor, in many cases, can one determine what teaching machines will accept the programs. Thus the listing serves only as a starting point, and for more complete information one must contact the program producers. A more recent catalog by the Center for Programed Instruction, entitled *Programs, '62: A Guide to Programed Instructional Materials Available to Educators by September 1962*, contains more detailed information on many of these programs. *(CP,* Sept. 1962, **7,** 354).

Program producers and compilers of future program directories could do a distinct service to potential program users by publishing a table of contents for each available program. In the present directory one finds a 321-frame program on "Elementary Statistics" listed immediately below a 1700-frame program on "Introductory Statistics." The reader may reasonably suspect that both programs cannot cover the same topics at the same level of detail, but the exact coverage remains a secret until one sees the actual programs.

WHEREAS the directory will see general use as a convenient reference listing of teaching machines and programs, most of the remaining sections of the paper will probably be of interest to a more restricted group of specialists in the audio-visual and automated

teaching fields. These additional sections, while stimulating in their own rights, might better have been published separately from the more detailed directory.

One section contains a rather philosophical discussion of the relative roles and importance of the teaching machine and the instructional program. The authors present a convincing argument that the program and the machine should not be regarded as separate parts of the developing instructional technology, but must be considered together as a unity. This position is contrasted by the authors with Lewis Eigen's machine-program dualism.

Perhaps the most fascinating portion of the entire paper is the Prologue, which contains a quotation from Edward Thorndike's *Education*, written in 1912. In this quotation Thorndike anticipates many of the developments now occurring in programed instruction. He discusses the drawbacks of conventional textbooks, pointing out that "Text-books often state what habits are to be formed without giving the reader exercises in forming them—". He goes on to advocate a method in which the student is required to try to solve problems for himself before receiving prompts. Thorndike's powers of prognostication are well shown in the following quotation: "If, by a miracle of mechanical ingenuity, a book could be so arranged that only to him who had done what was directed on page one would page two become visible, and so on, much that now requires personal instruction could be managed by print."

The present paper represents a distillation of the programed instruction and teaching machine industry of 1962. Its directory is impressive in total number of programs and machines listed, but disappointing in its lack of information about what these programs and machines are designed to accomplish and how they are to be used. Through its own strengths and weaknesses the paper reflects both the successes and the failures of the industry in converting Thorndike's aspirations to practical reality.

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ON THE CHARACTER OF METHODS FOR STUDYING CHARACTER

The research findings reported in *The Psychology of Character Development,* by Robert F. Peck and his associates, are important news *(CP,* June, 1962, **7,** 223-24). The book reports the first scientific study which has related systematically major dimensions of personality to elements of moral behavior across the full range of a normal population. Furthermore, this is the first time that such dimensions of personality and character have been shown to be linked to specific characteristics of family interaction. Consequently, I agree with the reviewer in the *Journal of Projective Techniques,* "This is a book which might well serve as a model for other longitudinal researchers and it ranks close to Piaget's work in the field of moral development."

Justin Aronfreed's review in *CP* was written from a somewhat different point of view. One can find in his paragraphs an opposition to (1) longitudinal studies, (2) reliance upon ratings, (3) multivariable research designs, and (4) the observation and correlation of naturally occurring events. What methods remain for the present study of molar aspects of behavior?

Although a difference in methodological preference is probably the crux of the matter, one or two serious misimpressions about the contents of the book need to be clarified. Out of some 3,000 pages of behavior records of various kinds, the study distilled several major components of behavior such as "ego strength," "superego strength," etc. These factor labels are unavoidably general, to be sure, but they were measured empirically. Moreover, meeting Aronfreed's criteria head on, their "antecedent threads (were) carefully sought," and found, in measures of family interaction, taken independently several years previously. To my knowledge, this is the first empirical validation of the neoanalytic theory of personality development in the normal range. To many, such an outcome is matter of very considerable importance.

As for the reviewer's questions about the rating system, the rationale is discussed on pages 237-243 of the book. In Table 19, evidence is presented which shows that other ratings on the same cases, made *without* discussion prior to rating, reached the same high level of reliability as the ratings used in this study. This evidence was not mentioned in the review.

In commenting on the case study on "Ralph," cited by the authors to illustrate the "rational altruistic" pattern of moral behavior, the reviewer failed to mention several pieces of evidence demonstrating "Ralph's" autonomy and nonconformity, evidence having direct relevance for the reviewer's own interpretation of the case. For example, the book carefully reports on an incident in which the boy corrected his school principal for bawling him out unfairly. Furthermore, adults in the community evaluate Ralph as a boy who "will stand up for what is right; although he tries to avoid conflict with others, he strongly resists other adults and children if he feels anyone is being mistreated." Thus, contrary to the reviewer's "impression" of "irrationally conscientious" behavior, there is ample evidence in the book to support the researchers' interpretation in terms of "adherence to abstract moral principles not supported by one's peer group" as an example of rational altruistic behavior.

I know that the authors would heartily agree with me, and with Aronfreed, that it is desirable to make studies which test experimentally specific, limited segments of moral behavior. To date, however, such studies have resulted in the same shortcomings which Hartshorne and May pointed to in their own research. At least in the exploratory era, which is where our present knowledge of character formation places us, we need the more complex, macroscopic type of study, such as that illustrated by *The Psychology of Moral Character,* in order to arrive at statements of what is probably true or demonstrably false. With the kinds of insights and theory developed from the book, experimental work can be carried on much more effectively.

CARSON MCGUIRE
The University of Texas

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By ELIZABETH DUFFY, *The Woman's College of the University of North Carolina.* Emphasizing individual differences, this book sets forth a group of interrelated hypotheses in regard to "activation" and supports them with relevant literature. *1962. 384 pages. $7.95.*

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By EARL B. HUNT, *University of California, Los Angeles.* Using the findings of psychology, logic, and cybernetics this book examines the problem of how any intelligence, human or artificial, can learn to assign objects, situations, or persons, to pre-existing classes. *1962. Approx. 296 pages. Prob. $6.95.*

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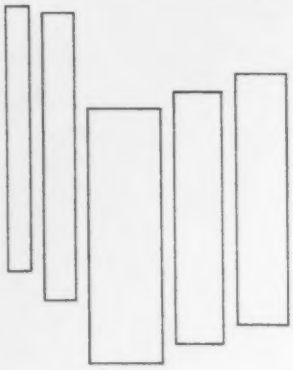

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